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LECTURES BY M. DE BLAINVILLE ON COMPARATIVE OSTEOLOGY.

THE COMPARATIVE OSTEOGRAPHY OF THE *SKELETON AND DENTAR SYSTEM,* IN THE FIVE CLASSES OF VERTEBRAL ANIMALS, RECENT AND FOSSIL.

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WITH GRAPHIC DELINEATIONS BY M. WERNER.

EDITED (FROM THE FRENCH) AND ADDITIONALLY ILLUSTRATED WITH NUMEROUS NOTES, OBSERVATIONS, AND DRAWINGS,

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M. DE BLAINVILLE has prefaced his discourses with the following prospectus of his views and intentions in preparing them for publication:—

THE very serious errors into which paleontology (often as ill conceived as poorly executed) has led many geologists, have induced me to attempt this work. Its aim, as indicated by its title, is first, to furnish the osteographist, and especially the paleontologist, with the means of recognising in a solid organ ("piece solide;" bone, using the term in its most extensive meaning), whether recent or fossil, what part of a vertebrate animal it has formed, and of ascertaining, satisfactorily (provided the piece submitted to examination be somewhat characteristic) to what class, order, family, or genus, and even to what species the animal belonged, and whether that species differed or not from any existing living species. In a word, the work is intended to form an osteography for the use of paleontologists, differing both in its plan and execution, from those of Messrs. G. Cuvier, Parkinson, Pander, and of Alton. Such a work appears to me still to be wanting to science, notwithstanding the long expressed efforts and wishes of Esper on this subject.

But, secondly, I frankly admit that I have in view a much higher object even than this. A comparative osteography and odontography of the five classes of vertebrate animals (as well recent as fossil) seem to be the only means of convincing the incredulous in science, and proving, by demonstration, the distinctions which exist in animal species, and their arrangement in a series.*

Finally, it appeared to me, that a suitable osteography might serve as a basis for com-

* *Note by Dr. Knox.*—The lapse of time passing for nothing with modern geologists, Lamack, Geoffroy (St. Hilaire), and others, took advantage of such views, by proposing a doctrine in which the specific distinctions of animals was set aside. The fossil Sawrians became in their eyes, the progenitors of the recent crocadile; and although such theories were altogether *speculative*, and positively contradicted by all human experience and chronology, yet both being, as it were, but a drop in the great ocean of time, these distinguished naturalists conceived themselves to be at liberty to set aside all human experience and human records, and appealed to the obvious changes which have taken place in the surface of the globe, as indicative of an immeasurable antiquity, and of a sufficient lapse of time and change of circumstance to effect all manner of alterations in the primative creation. This view is partly supported by many geologists, even in this country, but in a more covert way. They do not, for example, venture positively to deny that there have been *successive creations* of animal beings, but they assert what amounts nearly to the same thing, namely, that all the extinct and fossil species of animals have become so extinct and fossil, as the consequence of changes which are *now in operation* on the face of the globe. They thus imply, although unwilling to express it, that there was but *one* creation of animal bodies, this being the same doctrine as that which is maintained by Lamack and Geoffroy (St. Hiliare), but expressed with a caution that is peculiar to this side of the channel.

parative osteology, whose nature may be best understood by calling it the *signification of the skeleton*,* on which subject I delivered a special course of lectures in the Museum of Natural History, in 1835.

The pencil ought to furnish, in the execution of this work, a powerful laid to description, both so peculiarly combined, as surely and speedily to attain our object, for which we required figures of large dimensions, whose proportions were to be carefully reduced by means of the diagraphe, and so disposed and arranged as to render the comparisons that were to be instituted, not only easy, but developing the essential parts. This I think I have obtained by the employment of a conscientious and skilful artist, and forming five series of plates; 1st, for representing the entire skeleton; 2nd, for the entire heads; 3rd, for the characteristic parts, carefully selected; 4th, for the dentar system, comprising the roots of the teeth and their alveoli, which had never previously been done alone; 5th, for the fossil bones, and for the figures of animals transmitted to us by the ancients.

The descriptive part will be as brief as possible, without neglecting essential points of comparison; these, indeed, will be clearly pointed out; for which purpose I have selected in each great Linnean genus, a species, occupying the *middle* of the series, to serve as a type for the rest. The skeleton and dentar system of the species so selected, will then be given in detail, afterwards proceeding in the same way with the other species, ascending and descending from the type chosen, with references chiefly to the more important points.

With a view of rendering our work more generally interesting, I shall add to the purely descriptive part a historical and critical section, in which, before describing the fossil remains of each group (if any such exist) I propose to show how zoologists have classed those remains, their actual geographical distribution, and the means by which, for what length of time, those fossils have been known to men, and how our knowledge of them was obtained.

Undoubtedly the work will require, for its execution, a considerable period of time, but I venture to think that I occupy a position the most favourable for any labours,—one, indeed, which is superior to any other, with respect to my being able to conclude such a work in the shortest possible time, having, at my command, the osteological collection of the Museum of Natural History of Paris, the richest of all European collections; the fossil remains described by M. G. Cuvier, almost doubted since his description of them, by the collections of M. l'Abbe Croizel, in Auvergne; those of M. Lartet, in the environs of Auch; and good models in plaster of all fossil remains which have been discovered of late years in India, America, and Germany.*

Finally, whatever may be the extent of my work, it can never be incomplete, since, in fact, it may be considered as a series of distinct memoirs, many of which have been already read to the Academy of Sciences.

OF OSTEOGRAPHY IN GENERAL.

Under the term *bones*, *ossa* (*οστεα*), which form the subjects of osteography (a compound word, which signifies, in fact, *a description of the bones*), is comprised, in common language, all the hard parts which usually enter (whatever be their form or situation) into the composition of the bodies of animals of the first type in the zoological series; a type which I formerly named "osteozoaires," but which is better known by the name of vertebrate animals. The teeth, also, even to this day, are still reckoned as bones, by too many anatomists, as well as all the solid portion which may be developed in the skin, or in any other part of the body. Nevertheless, in the actual state of scientific organology, the term *bones* ought to be reserved, exclusively, for those organs (*pièces*), more or less hard, which constitute the passive part of the locomotive apparatus, forming, by re-union, the *skeleton*,—when situated in the muscular layer, subjacent to the skin,—or the *sklerette* when situated in the integument, or in the dermis itself. And thus we ought to view in the hard parts of this elevated division of the animal series, *six distinct circumstances*, requiring as many divisions, viz.:—

1st. The hard parts of the skeleton, or the bones, properly so called, and the sesamoides, bones of tendons, osteïds.

2nd. The hard portions of the skin, or the dermos.†—(*Dermatostea*, bones of the skin.)

* The reader, perhaps, need not be told that the term *signification of the skeleton* belongs peculiarly to the philosophic or transcendental anatomy, and that with respect to this subject, almost as many different views are held, as there are distinguished comparative anatomists in Europe. These views will come to be discussed by and by, but as M. De Blainville promises to supply a purely practical work, so, the translator will endeavour to render his notes equally practical.—*R. K.*

* The translator also hopes to have it in his power to avail himself of most of the fossil collections in this country.

† The predeliction of this illustrious anatomist for the employment of Greek, and, consequently, of new terms in science, is very remarkable. The reader will be pleased to remark this fact, and to be on his guard respecting the true meaning of these new terms. By *dermos* M. De Blainville here means a dermoid or integumentary skeleton,

3rd. The hard parts which render solid the first envelop of a sensorial bulb.—(*Bulbostea*, bones of the bulbs.)

4th. The hard parts which are external, or visible externally, as the teeth, the rings, *boucles*, which I shall call *phaneros*.—(*Phanerostea*, visible or external bones.)

5th. Those of an internal deposit, *phanerique*, but not visible on the *petros*.—(*Petrossa*, internal or phanerique bones.)*

6th. Some other organs, equally solid, developed in other parts of the organism, and to these we may give the name of *enderos*, or *enternos*.—(*Enderostea*, or profound.)

These solid parts of the organisation being capable of preservation for a longer or shorter period in the bosom of the earth, are, in fact, pretty nearly the only parts, the study of which has become a portion of paleontology, and, even then, in very different degrees of importance, proportional to the plan which they occupy in the general organisation, the skeleton and the teeth being the more essential as they most abound. On the other hand, the dermos, dermatostea (bones of the skin) occur but rarely, while the petrossa and *profound* still more so.

MAMMALIA.

OF THE BONES COMPOSING THE SKELETON.

We have just said that the skeleton is the *ensemble* or *whole* of the parts (more or less solid) or bones, which serve as a support to the frame,—as levers to the locomotive powers,—and which, forming a system, or an evident whole, so long as its connections have not been destroyed by artifice or death, occupies the inner surface of the cutaneous envelop, of which it is always more or less independent. Thus, a bone is a hard part of the skeleton, more or less separate; more or less mobile; affecting a position and a form which is determined by its employment in some act of locomotion. The bones of the skeleton,—the passive part of the locomotive apparatus,—are never completely separate and independent of each other, but are united more or less closely by fibrous bundles, which vary in strength. These are the ligaments, the study of which constitutes what is called in anatomy, *syndesmology*, and is unquestionably very important, as well to the surgeon as to the physiologist, but their study is pretty nearly useless here, because, as the traces of insertion of these ligaments present but seldom any durable character, their consideration has been very generally neglected in paleontology.

The Articulations.

But it is quite different with those surfaces which are termed *articular*, and which constitute the articulations, the solutions of continuity of the skeleton, the intervals where the movements take place. The modifications of these surfaces have always been reckoned as important. They present distinct forms, according to the extent and direction of the motions which they permit, the statical disposition of the body, and the action of the accompanying muscular forces. Hence the articulations have received particular names, which we must define.

The articulations are distinguished according as they are completely immoveable, almost immoveable, semi-mobile, and moveable, which is in relation with the degree of discontinuity of the conjoined bones.

In the first case the articulation has almost completely disappeared, excepting externally, by the progress of age and of ossification, which, having encroached on the membranous or fibrous interval of separation, has united the bones together. Clear examples of this may be found in the pelvis and cranial bones. In the second case, the connection takes place either by reciprocal locking (by small teeth and notches), or by the application of the articular edges, constituting *harmonia* in the first case, and *squamosa* in the second. But this latter kind of articulation, which persists only so long as there remains an intermediate soft part, passes easily into the first by the progress of ossification.

The semi-moveable articulations belong to the same section as the preceding; that is, between the two bones which are in connection, there is not, strictly speaking, solution of continuity, but the two bones touch each other much less, separated as they are by a fibrous intermediate substance, more or less soft, in which the motion is lost, which may be more extended, and exist at all ages.

But in the third kind of articulation, essentially moveable, and named *diarthrosis*, there exists a real solution of continuity more or less extensive, and at this point the bones are covered with a peculiar fibro-cartilaginous substance (cartilage of incrustation), smooth, and invested, in the fresh state, with an arachnoid membrane, called *serous* (synovial), with synovial glands, constituting a synovial apparatus:

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as distinct from that formed in the subcutaneous muscular plan of the body, which latter constitutes the true skeleton of all anatomists.—R. K.

* M. De Blanville here wishes to characterise more clearly, and to bestow distinct appellations upon, parts which heretofore have been named by most anatomists in a very vague and ill defined manner. The hard parts protecting, and, as it were, encrusting the extremities of certain of the nerves, he is disposed to view as specific and peculiar to them. The calcareous deposit, for example, constituting the larger mass of the petrous portion of the temporal bone, would, according to his views, form no part of the skeleton, but be included under the head of *petros*, or one of the hard parts encrusting the extremity of a sentient apparatus.—R.K.

The *form* of these articular surfaces, and, consequently, of the extremities of the bones which they terminate, varies considerably, and also determines the extent and direction of the movements. Three principle kinds of moveable articulation are more especially noted; but besides these, there exist many others, which, however, are much too diversified to be considered in a work of this nature.

We call that articulation a *ball-and-socket-joint*, in which the extremity of one of the bones that is in connection, has the form of a projecting sphere, and that of the other is hollowed into a cavity of the same form. This is the *arthradea* of surgeons. The joint is called *condyloid* when the extremity of two bones, or of two groups of articulated bones, is oval; concave with respect to one, and convex as regards the other.

Finally, the third kind is the *gyiglimus*, in which there is a reciprocal penetration of eminences and cavities between two extremities of bones, or sometimes of two eminences and two cavities together; in both cases the motion must be strictly angular.

The form and the direction of these different kinds of moveable articulations are of the highest consequences in surgery and physiology, as we have already remarked. But they are of no less consequence in osteography and paleontology, because they vary but little in the same species of animal. Besides, as they are in harmony with the rest of the skeleton, it may be seen how, from the existence of such and such an articulation between two bones, a conclusion may be drawn with sufficient accuracy respecting other portions; and thus we may admit or reject the analogy (correspondence) of a fossil fragment, with its analogous bone in a recent animal.

The Varieties of Bones.

Considered especially, the bones have been divided according to their *general form*, and their *position*, in the skeleton.

Under the first head, anatomists speak of them as being long, flat, and short, which easy definition may as well be admitted into paleontology as into anatomy. The long bones have one of their diameters much exceeding the others. In the *short* bones their three diameters more and more resemble each other; and in the *flat* bones, two, or at least one, of the diameters, is much shorter than those of the others.

The *flat* bones enter generally into the composition of cavities; the *short* bones are those found in the axis of the trunk, and in certain parts of the extremities; whilst the *long bones* are exclusively employed in the formation of the limbs.

With respect to the *position* of the bones in the skeleton, they may be arranged into those which are *median*, or in the axis of the body, and into *lateral* bones, placed on each side of the median.

The *median* bones are always symmetrical, *i.e.*, are formed of two sides or halves, which are perfectly similar at all ages. They are further distinguished into those which are above, and those which are below the intestinal canal, or with reference to the axis of the body.

The *lateral* bones, on the contrary, are never symmetrical; that is to say, they are not divisible into two equal halves by a longitudinal line, but are always arranged in pairs, the one right, the other left. In other respects they bear the strongest resemblance to each other, with the difference, only, which arises from the circumstance of their not both belonging to the same side.

The Median or Symmetrical Bones.

The *median* bones constitute, essentially, the basis or trunk of a vertebrate animal, and the *lateral* bones form the appendages, simple or complex, free or attached.

The consideration of the *symmetrical* or *non-symmetrical* nature of a bone is of the highest importance, and, consequently, the first point to be determined, when a fragment of a bone is presented to a comparative anatomist or a paleontologist. If the fragment be symmetrical, which is easily determined, then it is a vertebræ or a *sternebra;* if it be not symmetrical, then it is a bone of the lateral appendages, which is sometimes not so easily determined. For example, in the case of the bones which form the middle finger.

The Bones of the Spine.

Of the different portions which enter into the composition of the skeleton, the median (symmetrical) bones, placed above the axis of the trunk, and which have received the name of *vertebræ*, are evidently the most essential; so much so, indeed, that zoologists have derived from them the characteristic name of the entire first great division, or type, of the animal kingdom. In fact, almost all the *other* parts of the skeleton may be wanting, or not developed. The spinal column is alone constant, because, whilst at the same time that it forms a part of the locomotive apparatus, it serves as an envelop, or protecting case to the central portion of the nervous system in this type of vertebrate animals.

A *vertebra*, considered in a *general* way, and, consequently, in its highest state of development, complete, is a short median symmetrical bone, having a body (the principal part of the vertebra) to two opposite surfaces of which (viz., the external or dorsal, the internal, or neutral) is applied an arch that is more or less developed, whence result two canals, one dorsal, the other neutral.

The proportion of these three parts is in a direct ratio with the development of the

organ to be protected, that is to say, with the nervous system above, and the vascular system below, and with that of the locomotive apparatus. When the locomotive apparatus is at its *summum*, or greatest perfection, the body of the vertebra reaches all the development of which it is susceptive, so that nothing of it may remain but the body, as happens in the caudal vertebræ. Moreover, this body is never entirely wanting, even although the superior or inferior arches may have reached their *maximum*, as we see in the cephalic vertebræ, with respect to the first, or superior arches; and in the coccygeal vertebræ, with respect to the second, or inferior.

The vertebræ, whose series constitutes the vertebral column, are united to each other in a line, chiefly by their bodies, and in a variety of ways; *i.e.*, sometimes immoveably fixed to each other, as in the composition of the head and the sacrum, or semimobile, by means of a substance passing from the one to the other; or, finally, completely moveable, by arthrodia or by condyle.

Besides this principal mode of articulation of the vertebræ with each other, they may also join by means of the dorsal osseous arch, whether through the medium of articular condyloid surfaces, and, therefore, moveable, or (as takes place constantly in the head) they may be united to each other by the whole circumference of the arch, and become, therefore, immoveable.

In considering the vertebral axis of the body of a vertebrate animal, it is obvious that the vertebræ composing it form two cones, opposed base to base, one much shorter than the other, having its summit anteriorly, constituting the head; the other, much more extensive, whose summit looks backwards, forming what we generally call the trunk; and as these vertebræ, by certain arrangements in form, correspond pretty well with those specialities in the external shape of the animal, which are designated by the names of head, neck, thorax, abdomen, pelvis, and tail, so the vertebral have been distinguished into as many kinds, by the names of cephalic, cervial, dorsal or thoracic, lumbar, sacral, and coccygeal or caudal; a distinction which is by no means equally obvious in the five classes of vertebrata, and may be best seen, perhaps, in the first and last.

However this may be, we generally, in characterising, assume the human form as a guide.

The *cephalic* vertebræ are characterised by their position, which is altogether anterior; by their decrease from behind forwards; by the smallness of their bodies in proportion to the size of the superior arch; and because they are always more or less articulated in an immoveable manner, as well by their bodies as by the arch.

The *cervical* vertebræ are characterised by their position between the preceding vertebræ and the dorsal or rib-bearing vertebræ; by their constant mobility, with the exception of some cetraceans; and by the base of their transverse process being perforated by a foramen, although this character is less exclusive.*

The *thoracic* or *dorsal* vertebræ carry ribs (costiform appendages), which are articulated with them in different ways.

The *lumbar* vertebræ follow these, and have no ribs, not even incomplete ones.

The *sacral* vertebræ† are distinguished from the lumbar only by their offering an articulation, that is, more or less solid to the pelvian extremities.

Finally, the *coccygeal* or *caudal* vertebræ terminate the posterior cone, decreasing always remarkably from the first to the last.

The *second* series of median or *symmetrical bones* (that series which is inferior to the axis of the body), I have called *sternebræ*. They are always less numerous than the vertebral series. They are formed simply of a body, or median portion, without osseous arches above or below.

They never articulate with each other in a moveable manner, scarcely even with semimobility.

They form two groups, which are constantly distinct; one anteriorly, under the base of the cranium, known by the name of

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* The translator, at so early a stage of this great work, is unwilling to embarrass the student of phylosophical or transcendental anatomy with any lengthened discussion on the true character of the vertebræ, and which, indeed, will be considered with more propriety at a later part of the work. He will here, therefore, take the liberty merely of remarking, that he has long been in the habit of demonstrating to his class, from a very extended series of preparations in the museum at Old Surgeons' Hall, that the cervical vertebræ, even in the highest class of the vertebrata, viz., the mammalia, carry distinct ribs.

† *Sacral vertebræ*, like *cephalic vertebræ*, are by no means common terms in anatomical works. M. de Blainville's definition also is much too vague, even at this early stage of his work. In those vertebrata where pelvis and pelvic extremities are not developed, as in whales, there exist, in reality, *no* sacral vertebræ. The sacral vertebræ are, in fact, distinguished from the lumber by being united, and by bearing ribs. The translator has for many years demonstrated the facts, that the first three sacral vertebræ are provided with costiform appendages, which, by their union, form the *facies auricularis* or sacro-iliac articular surfaces, and that the ribs are not (as Mr. F. J. Meckel supposed) merely the anterior roots of the transverse processes greatly developed.

hyoid; the other more or less posteriorly, constituting the *sternum*, the inferior element of the thorax.

Osseous Appendages of the Median Bones.

The appendages which are added to the lateral portions of these two series of median bones are called *maxillæ*, or jaws, in the head; *cornua*, or horns, with respect to the hyoid apparatus; *vertebral*, or sternal ribs, in the trunk; and, lastly, *extremities*, or limbs, where these appendages (more or less complex, according to their degree of development) are free at their terminal extremity.

The *jaws*, which are never wanting in this type, however deformed or rudimentary they may appear to be, are the appendages of the second and third cephalic vertebræ. Hence the names of upper or anterior jaw, given to the one, and inferior or posterior to the other. Their common character is, never to be free of the extremity, and being more or less complex; that is to say, formed of a certain number of bones, joined together by different kinds of articulations.

The upper jaw, generally the more immoveable, may join the cranium by three roots, an inferior palatine, a lateral zygomatic, and a third superior or lachrymal (frontal?). This jaw, when complete, is formed of three (four?) portions, placed on a line, or end to end, and receiving the teeth; these portions in man are called the internal pterygoid processes, the palatine, the maxillary, and the pre-maxillary or incisive.

The lower jaw, when most complex, arises at the sides of the third cephalic vertebra, by the temporal bone opposing it, by the ossicula auditus, in connexion with the pars petrosa or auditory bulb, is prolonged by means of the tympanic bone (os quadratum). The osseous circle of the tympanum terminates in two parts or branches of the mandible, the interior of which alone supports the teeth.

The *trachelien* appendages, more or less complex, more or less numerous, according to the number of the hyoid sternebræ (median portions of the hyoid apparatus), join more or less immediately under the last cephalic vertebra.

The *thoracic* appendages may be of two kinds; that is to say, they may spring from, or articulate with, the vertebræ, or with the sternebræ, and meet each other, or simply come in opposition, forming the true ribs in the first case, and the false ribs in the second; and these false ribs, as they are called, may either descend from the vertebræ, or ascend from the sternebræ.

The free appendages, or *extremities*, as offering important considerations in indication of the elevation in the animal series, never exceed two pairs; one anterior, the other posterior, and more or less removed from each other; but in the three last classes of vertebrata they may be entirely wanting

In their most perfect state they are composed of four parts. The osseous girdle; the shoulder, anteriorly; the pelvis, posteriorly. The peduncle; the arm, anteriorly; the thigh, posteriorly. The manche; the forearm, anteriorly; the leg, posteriorly. And, finally, the terminating part, or instrument; the hand, anteriorly; the foot, posteriorly.

The *shoulder* and the *pelvis* are never composed of more than three bones; the scapula-ilium, the clavicle-pubis, the pre-iskion.* But either apparatus may be reduced to two, or even to one.

In the *arm*, as in the *thigh*, we find but a single bone, the humerus in the one, the femur in the other.

In the *forearm*, as in the *leg*, there are never more than two bones. They are placed parallel, and are the radius and the tibia, the ulna and the fibula.

Finally, the instruments, which are the *hand* and *foot*, are always more complicated, since, besides three groups of bones, placed the one before the other, (first, the carpus and the tarsus; secondly, the metacarpus and metatarsus; and, thirdly, the phalanges;) these groups are themselves repeated laterally, and, consequently, augment in number with the multiplicity of the movements.

Following out the original plan of my work, I have thought it my duty to limit what I had to say to these general osteological facts, to which, no doubt, might be added considerations—1st, on the chemical composition of bone, in which the solid part is always formed in a great measure of the phosphate of lime, and of a certain proportion of the carbonate; 2dly, respecting the ossification, which, commencing in the body of the bone, and at its two extremities, has led to the distinction of *epiphyses* and *body*; 3dly, respecting the anatomical structure of bone, permitting us to distinguish bones into *cellular* and *tubular* or *ivory*, according as the marrow which they contain internally is equally spread through their interior, or collected in a mass in the middle of the bone, giving rise to a canal that is more or less considerable in capacity, or which is altogether wanting, from the extreme density of the bone itself.

We might also have delayed for an instant to treat of the changes of distinction and separation, and especially of the form and proportion which the bones experience

* The shoulder, in birds and in some mammalia, is composed of three portions, as will be more fully explained hereafter, but the translator is not aware that to any of these portions the name of *pre-iskion* has ever been given.

with age, and according as the individuals whence they have been taken have attained the maximum of their development and strength, by a happy union of every favourable circumstance, and the absence of those circumstances which have an opposite tendency; but as each class of the vertebrata presents, under all these relations, differences that are extremely striking, it has appeared to me to be preferable, with a view to avoid useless repetition, to reserve what I have to say—briefly, however, on all these points—to the commencement of the osteography of each class.

I shall now rapidly sketch the other kinds of hard parts which enter into the composition of vertebrate animals, and which, consequently, may be found in a fossil state; but I shall first devote a chapter apart to a certain class of bones, certainly not numerous, and which, though appertaining to the locomotive apparatus, do not, in reality, form a part of the skeleton.

Bones of the Tendons or Sesamoïdes.

These bones, usually termed *sesamoïdes*, by the human anatomist, from their resemblance to a grain of Indian corn, and more lately termed by myself *osteïdes*, differ from the *bones*, properly so called. They may be regarded, however, as the free epiphyses of these bones, and, in fact, it is in the tendons, and at the point where these tendons are inserted into some bone of the skeleton, for the increase of muscular action, that these osteïdes are met with.

These osteïdes, moreover, are naturally subdivided into those of the trunk and those of the limbs, which, again, are subdivided into anterior and posterior.

Bones of the Skin.—Dermos, Dermatostea.

I have already said that I comprise under the name of *dermatostea*, or bones of the dermis, the solid parts that sometimes enter into the composition of the integument, which render it solid in its most important part (that is, the dermis), and which, in my *System of General Anatomy*, I called *sklerette*.

This kind of solid parts is but rarely found in vertebrate animals, whilst it is almost *characteristic* of the entomozoaria. Nevertheless, we shall have occasion to observe them in a small number of the mammalia (the armadillos) in some families of reptiles, as the crocodiles and scingues (gallywasps), and in some species of fishes, of different kinds.

The nature of these dermoid bones is quite similar to that of true bone; that is to say, they are formed of more or less calcareous matter, in the state of phosphate and carbonate, combined with a gelatinous or mucous animal substance.

Their structure has also a certain resemblance to that of bones, with this important difference, however, that it is never cellular and reticulated, and that, therefore, the dermatostea are without marrow.

With respect to the general form of the dermatostea, they are very commonly polygonal, smooth, or almost so, and slightly concave internally. They are, more or less, rugous and sub-convex externally. Their edges are sometimes denticulated, when they articulate with each other; at other times they are entire, when they assume a squamiform or imbricated character.

These bones, separated from each other by flexible intervals, permitting motions which are generally not very extended, are grouped together in very different ways, so as to form a kind of cuirass, buckler, or shield, breast-plates, greeves, for the thigh and legs, being more or less extended and distinct. And they are limited, occasionally, to certain parts of the skin. In others they form a complete skerette, as in the *scingues*, amongst reptiles, and the *lecoffres*, amongst fishes.

Bones of the Bulbs.—Bulbostea.

We have already seen that the normal organisation of the vertebrata presents solid parts, which cannot be arranged under any of the preceding categories. I mean those which render, more or less, completely solid a sensorial bulb, as the eye and the ear; and as bones of this kind are evidently called upon to play a different part in the economy, it is easy to see that their form, chemical nature, and anatomical structure, will necessarily vary, and that they will have scarcely anything in common but their anatomical position. In fact, the part which renders the cornea (sclerotica) solid in certain classes of the vertebrata, may be cartilaginous, fibro-calcareous, or completely calcareous. It may even be entirely of an ivory character; for example, in the bone named os petrosum, or pars petrosa, forming a portion of the ear in the mammalia.

It may also be single, or divided into several regular or irregular portions, and these peculiarities will be pointed out in each class.

Visible or External Bones.—Phaneros, Phanerostea, Externossa.

I employ this term to designate the fourth kind of solid parts that are met with in vertebrate animals. It comprises the *teeth*, which are developed generally in the skin, lining the buccal cavity. Also the osseous rings, and other analogous productions, which appear on the surface of the skin of certain parts of these animals. The character that is peculiar to this kind of solid parts is, to appear *externally*, to a greater or less extent, a portion, being, however, more deeply implanted in the subjacent tissues.

Their *chemical* nature is still pretty nearly the same as that of the two preceding kinds,

only that the inorganic matter predominates greatly over the organic, and, moreover, the organic part is not in the same condition as in the skeleton, it not being essential in the primitive phaneros, as in the true bones, but in my opinion, rather, as it were, accidental, and acting as a bond or gluten to the other.

The *anatomical* structure of the phanerostea differs, perhaps, still more from that of the bones than its chemical composition. In fact, instead of being formed of the same cellulo-gelatinous or cartilaginous basis, in the meshes of which is deposited the calcareous matter (itself forming a nettwork of varying density, and containing medullary juices), the phaneros is composed of extremely thin laminated cones, partially enclosing each other, and re-uniting together in so intimate and complete a manner, that the result is a moneogenous, compact, brittle whole, covered, often, in its external surface, with a layer, varying also in thickness, of a matter that is otherwise produced, and otherwise arranged, since its fibres are perpendicular to the layers of the central nucleus.

The first described is the ivory of the "phaneros," and the second is the enamel; and, as it happens that several of these phaneros, or of their parts, are occasionally re-united into one, by another granular substance, this latter is called the *cement*. Thus formed, the tooth (or phaneros) is at its maximum of complication.

The *form of a phaneros*, as we shall presently see, in considering each group apart, is of very great consequence in *Zooclassie*, and, consequently, in paleontology; but at the present moment it is not necessary to speak of it otherwise than very generally.

To understand properly the general form of a phaneros, it is necessary to know that it is a dead part, and a product exhaled on the surface of a bulb producing it in organic continuity with the animal body, and implanted more or less profoundly into the dermis, and even into the subjacent tissues; and that, in consequence, the form of the producing bulb determines rigorously that of the product, or the phaneros.

Now, by the production alone of the layers of this product, applied successively, the one within the other, over the producing bulb (the only living part), the only part united by the vascular and nervous system to the rest of the organism—this bulb diminishes in volume at the same time with the producing power; so that there comes a period when the composing cones having ceased to grow in diameter with the bulb itself, they begin to diminish with it. When, finally, the bulb proceeds, gradually, completely to disappear, the arrangement of the layer, always decreasing in diameter, finishes by producing a cone, opposed by its base to that which has been produced during the period of its growth.

The first cone, simple or complex, is named *corona*, with reference to the teeth. It is the only one which is exterior, and which is covered with enamel. The opposing cone, equally simple or complex, is called the *root*, since, in fact, it seems to penetrate into the subjacent tissues, and into the bones, where they exist. And, finally, the point of junction in the *neck*. According to this view, which I have reduced to the simplest expression, the phaneros is evidently a body that is foreign to the organism, although produced by it, tending continually to be pushed outwards by the incessant action of growth in the bones, in a sinus of which the bulb was placed and was there developed. An important character of a phaneros is, then, its being deciduous, at a variable epoch, in the animal life; and in this respect it resembles the hairs. In the mammalia, during the course of their lives, there are two systems of teeth, quite different as to number and form, and of which, that of youth, termed *milk teeth*, forms a sort of *avant-coureur*, as happens, in fact, with respect to the hairy covering of the skin.

The phaneros, considered simply with respect to the difference of *their position* on the surface of the bodies of vertebral animals, are distinguished into *teeth*, into *spines*, *rings*, and sometimes even toothed, as upon the sword-shaped vomer of the saw-fish. These species, as well as the rings, are phaneros, which exist on the surface of the external integument, and are rarely fixed into the skeleton.

The *teeth*, on the contrary, are phanerostea, which are developed on the surface of the buccal integument (mucous membrane of the mouth), imbedded, or not, into the osseous tissue, and in this case deriving their names from the bones with which they have relation. Hence the term pre-maxillary, maxillary, and palatine teeth; frequently also from the uses ascribed to them, of cutting, tearing, and grinding the food, they have been called incisive, canine, and molar. But these details will come in better when we speak of the mammalia in whose teeth these differences in form are best observed.

The Internal Bones; Petros, Petrossa, Lithortea.

The fourth kind of solid parts of the organisation of vertebral animals is much less important than that of the preceding, since it comprises only those which, though produced like the phaneros, by a bulb, are never visible externally.

For a long time I called them *otalithes*, but I prefer that of *petros*, signifying the same thing, but being more in harmony with the denominations of the other kinds of solid parts. The term *otalithe* will, therefore, remain specific. Their *chemical composition* appears to be entirely calcareous, but only in the state of carbonates, at least in the *otalithes*. Their *structure*, in this case brittle, hard

throughout, is evidently formed of layers, completely concentric, and enveloping each other, the first formed being the smallest, and occupying the centre; the last formed the largest, occupying the circumference, absolutely like a gall-stone or urinary calculus. Their *form* is necessarily variable, and, without doubt, is specific; their position in the eye and in the ear, distinguishing them into *ophialmolithes* and *otalithes*. In the first case it is the crystalline; in the second, the stones or bones of the ear; the one differing from the other, at least as much with respect to chemical composition, as in their position in the body.

The Interior Bones; Enderos, Enderostea. Profound.

Finally, in order to complete our view of all the solid parts that are to be met with in the bodies of vertebral animals, I have been forced to form a fifth and last order, with a view to comprise those bones which are arrangeable in none of the preceding categories, and which are met with, scattered as it were, in the depths of the organisation; such, for example, as the bone of the exciting organ,* in a great number of the mammalia, and the bone of the heart equally common in some of these animals.

But, excepting in position, these hard parts much resemble the bones, properly so called, and especially the osteïdes, in their chemical composition, and even in their anatomical structure. The form of that of the penis is regular, although different for each species. That of the heart is always very irregular.

After this rapid sketch of the solid parts in the *osteozoaria*—a sketch, undertaken rather for the sake of defining the terms which we so frequently employ, than with any other object, I shall proceed next to examine each of these five kinds of bones in each class, and in each great Linnean genus, prefacing the first description in each class by some generalities.

Another mode might have been adopted, that, namely, of examining one kind of hard parts throughout the whole series, and so on, successively, for the others. But although this method would have given to each a complete independence, it seems to us that the order adopted is not only less inconvenient, but presents more advantages.

Note on the Osteoïdes.—M. De Blainville's chapter on the osteïdes of the skeleton (see p. 143) does not appear to the translator to be at all satisfactory. To consider these bones as the free epiphyses of the bones composing the skeleton, appears to us, to say the least, inadvisable. They are positively an important and interesting part of the animal organism; a provision, in fact, for *the increase of muscular action*. In 1831, Mr. F. Knox, in a paper read to the Royal Society here, showed the naucular or shuttle bone of the horse's foot, to be, in reality, the free epiphysis of the distal phalanx (nail, coffin bone), illustrating at once the remarkable manner in which nature adheres to an original type, or plan, by slightly modifying her original elements. At the same time the fact is shown that the epiphysis of a bone may not only remain free, in certain animals, but may be called upon to perform new and important functions in the economy of the animal. The osteïdes are invariably developed in an original elementary cartilaginous basis, connected with a tendon; but we possess, in the Museum, a unique case, occurring in the human subject, demonstrating that nature can and does form a substitute on extraordinary occasions.—R. K.

* The bone of the penis unites, when fractured, by osseous deposit, very similar to what takes place in the common bones of the skeleton. An example of this we have in the Museum, in the bone of the penis of the otter. Thus, happily, pathology supports the philosophic view of our distinguished author. The bone in this case has been broken across at about ⅔ from its distal extremity. In consequence of the obliquity of the fracture, the bone is evidently shortened ¾, and at the same time is considerably increased in its diameter, at once by the riding of the fractured extremities, and the new osseous deposit. The entire original length of the bone has been three inches; the total length of the otter from snout to tip of tail being 38½ inches.

COURSE OF LECTURES
ON THE
PHYSIOLOGY AND PATHOLOGY OF THE BLOOD,
AND
THE OTHER ANIMAL FLUIDS,
DELIVERED IN THE SESSION OF 1839-40,
AT THE
School of Anatomy adjoining St. George's Hospital,
BY HENRY ANCELL, ESQ., SURGEON.

LECTURE 2.—*The blood a vital fluid.—New argument for the vitality of blood from the organic blood corpuscle.—Illustration of the vital importance of blood in the animal economy.—The vitality of the blood opposed to localisation of disease.—Order in which the fluids will be considered.—Chyle: New microscopical observations on, by Mr. Lane.—Difference between chyle and blood, and lymph.—The chyle modified according to the diet.—Application of facts to dyspepsia.—Comparison between the opinions of the later Humoralists and modern Solidists on this disease.—Injurious tendency of reliance on the special pathology in indigestion.—Improvements suggested.*

Gentlemen:—The blood, constituting the greater part of the subject before us, advan-

tage will result, from ascertaining, in the first place, what kind of fluid blood is, and how far the phenomena which it presents, are to be referred to laws which preside over matter as it exists in living beings.

Natural philosophy teaches us that matter is passive. The actions which take place in the *inorganic* world, in consequence of the mutual influence of masses upon each other, at sensible distances, are referred to the power of attraction. Those which take place in consequence of the mutual influence of minute particles upon each other, at insensible distances, are referred to the power of affinity, and there are other powers exhibited by inorganic matter, as that of electricity. The actions, on the contrary, which take place in the *organic* world, in consequence of the mutual influence of masses, or of minute particles, upon each other, to the aggregate of which, the term life is frequently applied, are referred by physiologists to vital powers.

Since the last mentioned differ from all the former phenomena in kind, true philosophy teaches us to regard the vital power, as differing in kind also, from all the other powers to which matter is subject.

Regard must be had to the popular and the philosophical sense of the word *power*. The powers to which the phenomena of Nature are referred, are not inherent in matter, they are impressed upon it by some unknown cause. Attractions, affinity, and vital power, may be regarded as properties with which matter is endowed, and as such they are spoken of, as attractions, affinity, and vitality; the two first may depend upon the same cause, acting under different circumstances, or they may all depend upon the specialities of different substantial principles. We are totally ignorant of the nature of these powers; and in physiology as in physics, the only safe course we can take, is to drop all consideration of the real efficient causes. The word power, therefore, is employed in the popular sense, attributing the phenomena observed, to the things in which they are observed, as sanctioned by philosophers; and matter may possess these powers, whether they are made manifest by action or not.

From the earliest times there have been two opinions respecting the cause of organisation and vital phenomena. By the one, vital actions are ascribed to organic structure; according to the other, they depend on a living internal principle, distinct from the body. The argument for a distict *vital principle*, is, to my mind, so sufficient, and so conclusive, that I take, for granted, that every physiologist who has studied the subject, with that deep attention which its importance requires, must have determined in its favour. It is not that the laws of life at all times supersede those of chemistry and physics, although, for the greater part in a living being, they appear to be superior to them. We are subject to the laws of gravity, yet we exercise locomotion in opposition to that force. The moment matter makes a part of a living body, its motions and forms come under the influence of a new power, or this entirely supercedes the former powers with which it was impressed, and it takes on new actions and new forms. The moment matter loses the principle of life, the powers with which it was formerly impressed, have again their full force, and it resumes the actions and forms of inorganic matter. Of the nature of the principle of life we know nothing; we study it by endeavouring to make out its laws, and it is worthy of remark, as tending to reconcile all opinions for the purposes of philosophical inquiry, that it signifies very little in the abstract, whether these laws differ in kind from those which govern dead matter, or whether they are the same kind of laws, differing so widely in the complexity of their operations, and separated in their results by such a chasm, that the imperfect state of our knowledge prevents the connection being seen. In either case analogical reasoning, as respects the phenomena of living and dead matter, is restrained, if not entirely forbidden.

The vitality of any substance then, solid or fluid, is the power which that substance possesses of exhibiting phenomena under certain circumstances, which are called living; and the phenomena of life are actions and influences in living beings which cannot be referred to the human laws which regulate inanimate matter. It is of the first importance to determine whether the blood, as it flows in the undoubtedly living vessels, is to be considered as a fluid, exhibiting the phenomena, and subject to the laws of inorganic matter, or whether it is to be regarded as possessed of vitality.

The opinions which have been entertained at different periods upon this subject have partaken of the prevailing errors in philosophy and medicine; but they serve to show that the affirmative of this question has, at all times, been more or less generally adopted. The common sense of mankind is in favour of the vital importance of the blood; thus, there is a natural dread attending the loss of it, common to the youngest child and the most robust adult. This does not depend upon having previously witnessed its effects, but on an instinctive feeling of the danger which results from its waste. The common observations of "living water," "devouring flame;" the popular opinion, that "diseases of the blood" are frequent, and the customs of the Jews, and some other nations, confirm this assertion. In the sacred records, upwards of 3,300 years ago, Moses states that "the life of the flesh is in the blood," and the ancient Greek and Roman philosophers and poets, regarded the fluids of the body, and especially the blood,

as the immediate seat of the vital principle. Hippocrates, Galen, and Aretæus, adopted this opinion in their writings. Harvey, in his work *De Generatione Animalium*, advocated it very strongly. The following passage is quoted by the late Mr. Wilson, in his excellent lectures on the blood, published in 1819:—"Ideoque concludimus sanguinem per se vivere et nutriri, nulloque modo ab alia aliqua corporis parte, vel priore, vel præstantiore dependere." Harvey also describes it as the "*primum circns*" and "*ultimum moriens*" in every animal. Hunter brought forward new observations and experiments to prove that the blood contains "*materia vitæ diffusa*." Since his time many of the greatest names in physiology have advocated the same creed. This, also, may be urged of our contemporaries, as shown by the writings of many of those authorities quoted in my first lecture. Lastly, Müller, in one of the latest and perhaps the best work extant upon physiology, observes as follows:—"There subsists between the blood and the organised parts a reciprocal vital action, in which the blood has as large a share as the organ," and "the actions of the blood cannot, certainly, be comprehended from chemical and physical laws; therefore the blood must be regarded as living." To the contrary, then, of the opinions of those who have affirmed that to give life to the fluids "seems revolting to our common sense;" it appears that the instinct and the judgment of mankind, is in favour of the vitality of the blood.

We infer, *a priori*, that the blood is endowed with vitality, because the fluids constitute a part of organised beings, equally essential with the solids. There are living animals which sometimes contain only a few grains of solid matter to twenty or thirty pounds of fluid. Nearly all the organised solids yield, on chemical analysis, four-fifths of their weight of water. The reproductive material is as much fluid as blood, and it is manifestly vital. Solid and fluid are only relative terms, and if we direct our attention to the extreme capillaries, and beyond them, we must arrive at a point where the blood may be regarded as extravasated, and so divided and mingled with the materials of a tissue, that no positive line of demarcation can be conceived between the particles of the fluid blood and those of the solid. The blood has the same corpuscular structure during the life of the body, and furnishes the same chemical constituents after death, as the solids. The life of every part of the body depends on the blood; and although it has been objected that this only proves it to be necessary for animal life, and that food and air, to which we do not attribute life, are equally necessary, it may be replied, that life will continue a long time without food, whereas, without blood, it ceases instantly, and that air acts directly upon the blood itself. The fact of the cessation of life by the abstraction of air, may be taken as a proof that the life of every part depends upon the blood. Many other reasons, *a priori*, might be given in favour of the vitality of the blood.

That the actions which take place in the blood differ in kind from anything observed in the inorganic world, and depend on the same power which produces the varied phenomena of life, is proved by a variety of considerations. The molecular combinations in the blood, are equally distinct from the binary and ternary compounds of mineral matter, and strictly identical with the complicated atomic combinations in the living solids. The *fibrine*, which separates itself when blood is extravasated, either within or without the body, exhibits the characteristics of living action; its component particles are vesicular, and they assume a lamellated and areolated arrangement, and it possesses an animal contractility, which is destroyed only by those causes which destroy its vitality. The blood, moreover, is composed, in part, of *corpuscles*, which are seen, by the aid of the microscope, to possess a complicated organic structure. Mr. Lane lately pointed out to me the blood corpuscle of one of the lower animals; he had washed the colouring matter away, and it presented the appearance of a delicate pellucid *membrane*, loosely investing an opaque solid ovoid nucleus. Schultz states that air is contained within the membrane, and surrounds the nucleus during life, but Mr. Lane and I have been unable to satisfy ourselves on this point. No doubt, however, is left on our minds, that the human blood corpuscles, as well as those of animals, have all the requisites to constitute them organised structures, susceptible of living actions; and Schultz describes them as possessed of a very remarkable organic contractility, which obeys certain stimulants. Another proof of the difference between the actions which take place in the blood, and those which occur in inorganic matter, is found in the circumstances under which blood maintains its fluidity. Those who deny its vitality have said, "This power cannot be the cause both of its coagulation and its fluidity." But this is a mere sophism. Without meaning any analogy, but merely to show how the same cause appears to produce opposite effects, caloric is the well known cause of both the contraction and the expansion of water. The blood within its vessels maintains its fluidity by virtue of its living power, and the circumstances under which it is placed. The blood coagulates, when extravasated, by virtue of its living power, to answer certain obvious final causes. If to these proofs be added the fact, that blood evinces spontaneous movement, without any evidence of having derived it from the action of the solid

parts, as in the lower animals, where there are no containing vessels, and in the embryo and new growths, before the vessels are formed, the direct proof of the vitality of the blood, appears to me so convincing, that it is impossible any unprejudiced mind can reject it.

There are innumerable events in the animal economy, which are best accounted for on the principle of the life of the blood. The sudden death which results from the actions of lightning, blows on the stomach, gusts of passion, and the action of various poisons, as pointed out by Mr. Hunter, are of this character. No lesion of the nervous system, or any other organ, is left to tell its tale; but the blood is invariably fluid. Added to which, experiment has shown, that animal life is at least temporarily independent of the entire nervous masses, including the brain, spinal marrow, and sympathetic ganglia. These may be crushed, burnt, destroyed, suddenly or gradually, and yet life continues. Setting aside the question of primary or secondary agency, and that which respects the means of communication, can it be doubted, in such cases, that death is the result of the blood being suddenly deprived of its vital principle?

Dr. Wilson illustrates the vital importance of the blood, and the kind of fluid which it is, in the following words:—"The blood of the chemists is not our blood, which is the blood within our own vessels; our warm, busy, ever-moving, ever-changing, blood. The blood of the physiologist is that which we feel at our own pulse; that which is exposed on every surface, and circulates through every tissue in succession. The elements of the blood may be furnished from the food and the atmosphere alone; from the lungs and the stomach; but the blood is elaborated as blood, all over the body, in one organ as in the other. It is the result of the combined vital actions of every nerve, and every set of vessels in the body, of every viscus, and every texture,—the refuse of every secretion. The blood only that has circulated through all the organs of the body, is fit to maintain the circulation, and to support life. Should any of the secretions be suspended, the whole mass of blood, sooner or later, will be vitiated. Thus, for the manufacture of good blood, urine, saliva, and bile, are as necessary as chyle. The absence of any one organ, or the deterioration of any texture, would spoil all the circulating blood. It is thus spoiled in fixed organic disease; and hence the increasing sallowness, the absence of the healthy tint, in those affected in this way, in whatever structure the altered growth or perverted action be found. The great assimilating structures of the lungs and stomach may continue sound— their function complete— and yet if any one of the minor organs forget its function, the blood will not the less be vitiated. Every organ, every texture, every small vessel, every set of vessels, all and every one have a double function to discharge—the maintenance of structure, and the production of blood. We know that this fluid, though replenished from the lacteals and other absorbent vessels, is not completed in them; there it is chyle or lymph, it is not blood. Where, then, is blood made? In no particular organ, but all over the body, for in all is it modified and altered. For, even admitting that it is fabricated in any particular organ, as in the liver or spleen, yet its perfection must depend on the perfection of the functions of all the other organs. Our common idea is, that all the organs and tissues of the body are necessarily engaged in spoiling the blood. I would rather on every account believe, that they are all engaged in elaborating and perfecting it, and that every texture, while it appropriates enough for its own wants, in doing so prepares what is needed by others. In all the systems of nature, this principle of *benevolence*, if I may so express it, is to be recognised by those who are willing to search for it. It is in the human body, as in the universe, and in the systems that compose it—the parts conspire for the advantage of the whole."

The evidence for the vitality of the blood is as convincing, to my mind, as that which establishes the existence of a vital principle; and I cannot help thinking, that whoever denies the one, must do the same of the other. Perhaps the two most direct proofs are to be found in the physiology of the fibrine, and of the coloured corpuscles, both which subjects will be fully entered into in future lectures. But it has been well observed by Mr. Palmer, that on considering all the circumstances attending this fluid, it is scarcely possible that we should think otherwise than that it has "life within itself;" and the same gentleman says, "The argument for the vitality of the blood is cumulative, and must be viewed in its totality, in order to estimate its real force."

The determination of this question was of the utmost importance as to our future inquiries, and I shall neglect no opportunity to elucidate it in the detail. The life of the blood has been denied or evaded by a great proportion of the pathologists of the present century. Some have admitted it, without being aware of the immense importance of the admission, in questions of health and disease. Others have scorned it as "abstract," "theoretical," "philosophical," anything but "practical." If it be a truth, it is entirely subversive of the dogmas of the special pathological school, and a libel upon its practice. We do not disparage the labours of those who cultivate morbid anatomy, and we are fully aware of the great value of a knowledge of diseased organs, in the history and treatment of a case. For my own part, I have never, since I have been in the pro-

fession, at home or abroad, neglected any opportunity to make post mortem examinations. But, I am firmly persuaded, that special organic lesions do not constitute the whole, or the most important part, of any case; that there is such a thing as general disease, in which the vital qualities of the blood are in a great degree involved; and that he who looks to the local affections only, or primarily, withholding the attention due to the signs and symptoms of the general disease, must be doomed to eternal mortification in his diagnosis and prognosis, and to the most lamentable errors in his practice.

The fluids of the human body may be regarded in four points of view:—1. Those which form the blood, comprising the chyle and the fluids absorbed directly from the digestive mucous surface. 2. The blood. 3. The secretions and exhalations which proceed from the blood. 4. The lymph which proceeds from the blood, and contributes to its renovation.

Directing our attention, in the first place, to the chyle: you are aware that substances which have undergone the influence of the living power, and have been organised, constitute the only proper solid aliment for man. The result of the action, of the various powers which are brought into play, and of the gastric juice, in digestion, is to reduce all kinds of food into a few *nearly* uniform principles. Perhaps, as regards a healthy subject, using wholesome diet only, we might affirm that the chyle, the fluid result of digestion in the first passages, is uniform in composition, but we shall find that it is liable to vary; and there can be no doubt that the habits of society are such, as to incur its incessant modification. The chyle, then, is the result of the action of various secretions from the blood, and various powers referable to the nervous system, upon our food; but we cannot study the process of its formation, advantageously, without being previously acquainted with the vital fluid from which these secretions proceed, and on which these powers depend. The ancients had an imperfect notion of the existence and nature of chyle, but it was observed anew by Aselli, at the commencement of the seventeenth century, and, having most frequently an opaque white colour, it was compared to milk. Boerhaave regarded it as an emulsion, or mechanical mixture of oil and water. Emmert and Vauquelin were the first modern chemists who furnished us with positive notions respecting its composition, and the original analysis has been confirmed and rendered somewhat more complete by Marcet, Prout, Magendie, Tiedemann, Gmelin, and Müller. But the experiments of these learned chemists are confined to the chyle of animals, and there is much reason to regret that no precise observations have been made upon that of the human subject.

The chyle is an opaline or milky fluid, contained in the lacteal system of animals during digestion. It has a slightly alkaline reaction. When procured from the thoracic duct, it coagulates spontaneously after removal from its vessels. It separates, like blood, into serum and clot, and at the same time a greasy matter most frequently collects on its surface. It is lighter than blood, and prone to acidity on exposure to the air. When viewed under a powerful lens, it is seen to be composed of minute globules, or rather *granules*, swimming in a transparent fluid, to which granules it owes its opacity and milky appearance. These chyliferous granules measure, according to Prevost and Dumas, $\frac{1}{1700}$ of an inch in diameter; according to Müller they are one-half or one-third the size of the blood corpuscles; Mr. Lane measured some procured from the lacteals of a dog, in my presence, which were $\frac{1}{1714}$ of an inch, the blood corpuscles of the same animal being $\frac{1}{3418}$ of an inch in diameter, according to which the bulk of the blood corpuscle is something more than 27 times larger than that of the chyle granule. Müller watched the coagulation of the chyle under the microscope, and found that it corresponded exactly with that of the blood. Delicate pellicles or clots, constituted in great part of a transparent substance, and, evidently owing to the coagulation of animal matter, separated themselves from the fluid, and connected the granules together. Chyle contains saline matters also, of which chloride of sodium is in the greatest proportion. The chemical substances, which have been shown most decidedly to exist in the chyle of animals after death, are the following:—albumen, fibrin, osmazome, extractine, yellow fatty matter, brown fatty matter, chloride of sodium, acetate of soda, carbonate of lime, acetate of lime, and, according to the most recent authorities, iron.

The first important circumstance respecting the chyle, is the great difference which it presents, according to the part of the lacteal system from which it is procured. Emmert and Vauquelin first observed that the quantity of fibrine gradually increases as the fluid proceeds from the coats of the intestines to the blood in the vena cava, and this observation has been amply confirmed. Its coagulability increases in proportion to the fibrine which it contains. That procured from the thoracic duct of the horse coagulates in about ten minutes after it is emptied from the vessel, but chyle has rarely been found coagulable before it has passed the mesenteric glands. The colour of chyle is also liable to vary in different parts of the chyle vessels, and at different periods of digestion, from an opaline to a milky, and from a greyish to a cream colour. This may be owing to a difference in the proportion of

its proper granules, or to the addition of other substances. A reddish tint has also been frequently observed in this fluid, more particularly as procured from the thoracic duct of the horse and some other animals; but the chyle is never found to be of this colour in the lacteals, before they pass through the mesenteric glands. This colour is sometimes very faint, and becomes deeper on exposure to the air. According to Tiedemann, chyle of a pink hue becomes scarlet when submitted to the action of oxygen gas, and of a blue or brownish red, approaching to crimson, in azote; and, from chemical analysis, he inferred, that it owes its colour to *a solution of the colouring matter of the blood*. Mr. Lane examined this coloured chyle under the microscope, and found that it is owing to the admixture of *coloured corpuscles*, which there can be very little doubt are *blood corpuscles*, conveyed into the chyle with the lymph, since they are found in the lymph of various parts of the system, and the red tint of the chyle is not observed until after the lymph is mixed with it.* Tiedemann and Gmelin submitted coloured chyle to the action of various gases, with a view to observe its absorbing power. When 80 measures of chyle were agitated with 70 measures of oxygen gas, 5 measures of the latter were absorbed: 20 measures of chyle agitated with 18 measures of azotic gas produced no absorption. By agitating 18 measures of chyle with 41 of carbonic acid, 11 measures of carbonic acid were absorbed, and the serum assumed a deeper yellow, and the clot a violet tint. The modifications which the chyle undergoes in its transit from the intestines to the vena innominata, are attributable in great part to the admixture of the lymph; they must be referred also, in part, to the hitherto unexplained action of the mesenteric glands, and we must not forget that its molecules are now brought under the immediate influence of the living principle, which pervades the containing solids of the animal economy and their fluid contents. The question has been put, "Is the blood alive?" I will transfer it by asking, "Is the chyle alive?" No doubt remains as to the first. There may be some difficulty in replying to the second. We see the most wonderful contrivances in the apparatus of digestion, for the purpose of rendering congruous the molecules of the food, and it is difficult to conceive congruous particles within the sphere of living action for one instant, without partaking of its power. Yet the phenomena of life are so inscrutable, that it is impossible for us to say when or where the material is to be regarded as assimilable. John Hunter was of opinion that the chyle is itself alive.

Mr. Lane has lately examined the lymph and chyle of the horse, from various parts of the body, microscopically, and has given me the opportunity of doing so likewise. A horse, doomed to be destroyed, was killed five hours after a hearty meal of oats and beans. The chyle within the intestine contained the well-known chyle granule; that of the lacteals before it passed the mesenteric glands was white like milk, and that of the thoracic duct was of a much deeper red than we had been led to expect. In fact, the exterior of the duct presented the aspect of a vein. On examination under the lens, lymph from the lower extremity, and the contents of the thoracic duct, contained an abundant quantity of coloured blood corpuscles, in an imperfect or deteriorated condition. The lymph of the superior extremity was slightly coagulable. The thoracic duct was examined a little more than an hour after death, and it should be remembered that the chyle continues to flow towards the heart for some time after that event. We infer, in this case, that the thoracic duct chiefly contained lymph; and without wishing to place too much stress on a limited number of observations, we think it questionable whether the chyle itself is ever coloured, but are inclined to believe, as before stated, that the pink and red tint which it frequently presents, is wholly and at all times attributable to the admixture of more or less lymph, containing blood corpuscles.

The chyle differs from the blood in its granules or globules being smaller, perfectly round, free from colouring matter or nearly so, and having no envelop, and no portion which is soluble in water. Its alkalinity is less marked, and sometimes wanting. It has a smaller proportion of solid matter. The proportion of fibrine which it affords is also much less, and this principle differs somewhat from that of blood; acetic acid dissolving only a small part of the coagulum of chyle, whereas it dissolves readily the whole of the fibrine of blood. In this respect the fibrine of chyle resembles albumen, and Vauquelin, and some other chemists, have regarded it as imperfectly transformed albumen. The chyle differs from blood also in frequently containing fatty matters in a free state, whereas these matters in the blood are in a state of perfect combination with other substances. The iron which the chyle derives from the food, and conveys into the blood, is in a less intimate state of combination in the chyle. It can be extracted by means of nitric acid, and even the serum of chyle affords traces of it,

* These observations, as far as they go, are a reply to the following question put by Breschet: "Is the colouring matter of the chyle and lymph dissolved, or is it dispersed in the same manner as in the blood globule?" It appears from this author, also, that the presence of "blood globules" in lymph had not been determined by microscopical examination.

whereas it is the most difficult thing imaginable to obtain the iron from the blood.

The differences between chyle and lymph are chiefly the following:—Lymph contains globules, but they are fewer in number, larger, and have a more transparent appearance, and a less granular aspect than those in the chyle. Both furnish fibrine, but this principle is in the greater quantity in the chyle than in the lymph. The saline particles are much the same in both. The chyle contains fatty matter in suspension, which the lymph does not; this fatty matter varying greatly in quantity in the former, and appearing in various sized globules, quite different from its proper granules. I am endeavouring, in this lecture, as much as possible to isolate that which relates to the chyle from that which relates to the lymph, but there is some difficulty in doing so. These fluids soon blend together in the living subject, and there is no doubt that the numerous discrepencies in the statements of authors arise from having omitted to distinguish between the pure chyle and the mixed fluid. The microscope furnishes the best means of making this distinction. There is an important observation also respecting the chyle and lymph, made by M. Collard de Martigny. During digestion the lymphatic system is comparatively empty of fluid, but when chylification has terminated, the lymph re-appears in abundance; in this manner the lymph and chyle alternately occupy the thoracic duct, which vessel is never empty.

It is matter of the greatest moment to know whether the chyle, which is one of the pabula of the blood, possesses precisely the same qualities under every variety of diet, or whether its qualities vary according to the nature of the food with which an animal is fed. Many statements were made and contradicted in the early period of experimental physiology, respecting the absorption by the lacteals of colouring matters introduced into the stomach. It appears, as a result of all the experiments, that it happens very rarely indeed that any substance which has not been greatly modified by the digestive powers, and somewhat assimilated to the nature of chyle itself, is taken into this part of the vascular system. But the following facts deserve our best consideration:—The chyle differs very considerably in different animals, and these differences respect the constitution of the species. In the horse, it has a greater quantity of fibrine. In birds, it is not milky, but clear. The size of the granule, according to Müller, bears a proportion to the size of the blood corpuscle. It is not very turbid in most of the herbivorous, but it is always turbid in the carnivorous animals; yet the chyle of the young of herbivorous animals, nourished with milk, is always more or less turbid. In the original experiments of Vauquelin, on the chyle of horses, he found that it varied in the quantity of fibrine which it contained, and consequently in its coagulability, and he referred this variability to the nature of the aliments upon which an animal is fed, or to a diseased state of the digestive organs. Marcet made some experiments to determine whether any chemical difference exists between chyle formed from vegetable food alone, and that formed from animal food alone, in the same species of animal. He analysed chyle procured from the thoracic ducts of dogs, previously fed for the purpose, and destroyed three hours afterwards. The chyle from animal food was whiter and more opaque, the coagulum had a more distinct pink hue, according to this gentleman, and the residue of the coagulum, after the separation of all the serum, putrified sooner than that from vegetable food, and it gave out more ammonia. The serous portions also differed. In the former description of chyle, a white opaque creamy substance rose to the surface on standing, which was not the case in the latter. Chyle from vegetable food yielded three times as much charcoal as that from animal food.

The solid matter obtained by evaporation was greater in the former than in the latter, and it yielded by destructive distillation more ammonia and more fixed oil. Thus, according to Dr. Marcet, the chyle from animal food has a greater resemblance to blood than that from vegetable food. Although less difference was found by Dr. Prout between the two kinds of chyle, still the difference was considerable. Tiedemann and Gmelin have made most elaborate researches upon subjects connected with digestion. On examining carefully the results of their experiments, it appears that very great differences occur in the chyle of animals, according to the nature of the food. They fed animals, also, on simple substances, such as butter, starch, gelatin, or albumen, and also on more compound materials. The chyle of sheep fed on grass or hay was very slightly turbid; in those fed on oats it was much more so. The chyle of dogs fed on butter was whiter and more milky than ordinary, and their diet manifestly surcharged it with fat; in fact, a great quantity of fatty matter could always be obtained by means of æther, from chyle furnished by fat nutriment; and the passage of fatty matters from the chyle into the blood, and its escape with the urine, is rendered very probable by the experiments of these gentlemen. In this manner they believe it to be prevented accumulating in the blood. In animals fed with starch, no trace of this substance could be detected by means of iodine, either in the chyle, in the blood of the vena cavæ or vena portæ, in the urine, or in the fluid of the pericardium; but in animals which had been nourished with this substance alone during nine days, sugar, or a substance analogous

to it, was obtained from the chyle, blood, and urine. Both the chyle and the blood differed greatly in dogs, according as they had been nourished on gluten, milk, bones, and other substances. Differences were also observed in the chyle as respects its coagulability, colour, proportion of coagulum and serum, the quantity of solid matter which it contains, and many other circumstances, and these differences had reference to the quality of the diet from which it was formed. Magendie's experiments have been confirmatory of these facts, and Breschet concurs in the general fact, that the lacteals absorb other organic materials of the alimentary mass besides the chyle; and sometimes, also, foreign substances unaltered, but always in a state of solution, as certain salts; or in a state of extreme division, as fatty matters. Tiedemann and Gmelin also found, in their investigations upon the effect of introducing foreign substances into the alimentary canal, that various salts are occasionally taken up and conveyed with the chyle through the lacteal vessels. It appears, then, that although, as a general principle, substances must undergo the process of digestion before they can be absorbed by the lacteal vessels, yet all the greatest authorities concur, that the chyle itself is liable to very extensive modifications, according to the kind of diet upon which an animal is fed; and although we have to regret that no observations have been made upon the chyle of the human subject, there is no reason to belive that this forms an exception to the general fact, and we are, therefore, constrained to admit that the chyle is liable to incessant changes of composition, according to the nature of our diet, which circumstance pre-supposes incessant changes in the composition of our blood.

The following facts further illustrate this fundamental consideration. By microscopical examination of the chyle, from the thoracic duct of a dog, killed five hours after a meal of *bread, milk and butter*, Müller saw globules of oil, of unequal size and perfectly transparent; he saw also the ordinary chyle granule. These additional oil globules have generally been found after the above description of diet. Mr. Lane and myself examined lately, with great care, the chyle of a dog killed five hours after a full meal of *tripe*; the chyle had a few, but very few globules, which were perfectly distinct from the proper granule of this animal fluid, and easily recognised. Admitting the accuracy of these observations, of which I have no doubt, it follows that, in the same species of animal, under circumstances as nearly as possible similar, the chyle varies in its evaporation, according to the nature of the ingesta. It should be remembered also, that the diet employed by Müller was not the most suitable to a carniverous animal, and it was therefore less likely to furnish a pure chyle

In consequence of some experiments made by Dr. Lewer, and many facts which had been observed relating to a milkiness of the serum of the blood, which was found to exist during digestion, and the passage of the chyle into the circulation; it was formerly believed that the chyle, when mixed with the blood, does not immediately change its nature. Boerhaave was of opinion that the change of chyle in the blood is not perfectly effected, until after the lapse of 24 hours, and that it retains the nature of the aliments from which it is produced while it flows with the blood. He represents part of the chyle received into the blood as lighter than the latter, and swimming on the top of it, and observed that by its gradual flow into the blood, we have a mixture of a little crude matter with a large quantity of perfectly formed humours. He considered the quality of the chyle poured into the blood to be of the greatest importance, and its impurity to be the cause of many diseases.

No doubt Boerhaave carried his views under this head too far, and that he adopted them too exclusively. He is evidently proceeding upon an erroneous conjecture, when he speaks of the chyle retaining the nature of the aliments, after it is mixed with the blood. But, since the time of Cullen, the facts have been set aside; although these very facts are confirmed by modern observers. We have seen that the chyle varies according to the nature of the aliments; I shall have occasion to point out, in future lectures, that the blood varies, also, according to the nature of the ingesta. Berzelius, Marcet, Thackrah, Christison, and others, have confirmed the fact, that a creamy substance, obviously derived from the chyle, sometimes appears on the surface of the blood, giving the serum first a milky colour, and then separating and rising to the surface. Many observers, whose attention has been particularly directed to the subject, and who are those, therefore, most to be relied on, assert that obvious variations in the appearance of the serum, resulting from the character of the diet, are much more frequent than is usually supposed. Marcet states, that the creamy matter is more frequent in persons using animal diet, than in those who live on vegetable food. I can state, also, from my own repeated observation, that the serum of blood drawn during the digestion of a full meal, is always more or less disturbed, and I know this to be confirmed by the experience of other practitioners. There was an experiment made by Mr. Lane, in which, also, I participated, bearing upon this part of the subject. A poor dog, doomed to end his mortal existence on account of an incurable lameness, was destroyed with prussic acid four hours after an enormous repast. We believe that we traced the chyle globule distinctly, throughout the lacteal system into the venous

system. We saw it mixed with the venous blood of the vena innominata of the right auricle, and of the pulmonary artery, and then again with the arterial blood in the arterial system. Led on by this result, we examined the blood of several individuals when fasting, and also of the same individuals at various periods after meals, and, upon the whole, we believe that we traced chyle globules, or imperfect blood corpuscles, mixed with the blood, and bearing some correspondence in number to the period which had elapsed after a full meal. To a small extent, these observations correspond with the statements made by Schultz, of which you will find an account at page 712 of the last volume of THE LANCET. Should they be confirmed, they will re-establish the old doctrine respecting crude matter getting into the living blood from the alimentary canal, and render it highly probable, that, as was formerly supposed, this is one of the most frequent causes of diseases. We do not attribute much importance to these microscopical observations at present; they are introduced partly to call attention to a very interesting matter of inquiry; but discoveries of this kind ought not to be adopted and applied until after the repeated corroboration of many observers.

There is another class of facts respecting the chyle, which we must bear in mind, but to which we must not at present attach too much importance. Their value is lessened on account of the great neglect which the pathology of the fluids has hitherto met with, and accordingly we find in various works, that the most interesting cases of the kind are most imperfectly described. I allude to those instances in which the chyle is said to have passed unchanged from the body with the various secretions, and particularly with the urine. Schenkins quotes the case of a young man, of thirty years of age, who daily made a considerable quantity of urine, depositing a white substance, like the curd of milk, sufficient to fill a common chamber utensil, besides the urine which was above it. But you must bear in mind that physiologists fell into the error at first, of regarding chyle and milk as identical, which has since been entirely disproved. Marcet, Elliotson, and Prout, however, record cases where chyle was found in the urine. Dr. Elliotson's case was that of a young married woman, whose urine contained very large coagula. She dined at noon; in the evening the coagula were white; in the morning, pale with pink streaks. She had been some months in this way, was in fair health, and had a good appetite. There were some general symptoms of dialute, but no sugar in the urine. Notwithstanding the fluid discharged seemed to present as much coagulum as urine, on drying, the quantity of chyle proved to be very minute. Dr. Elliotson seems to regret that this young woman would not allow any blood to be extracted from the arm. Had he been aware of a method pointed out by Mr. Lane, in a paper, with the manuscript of which he has afforded me a view, by which the minutest portion of its blood and the animal fluids may be examined microscopically, and preserved as specimens, I am sure it would not have been neglected in this instance. An observation made in this way, so as to determine whether the secreted fluid contained the chyle granule, and whether the blood contained any unusual particles, would have been highly interesting and important.

These are the principle facts of which we are in possession, respecting the chyle. It must be confessed, that considering the immense amount of talent and zeal which has been for many years employed, in the advancement of medical science, our information under this head is extremely scanty. There is no analysis of human chyle, that I am aware of, extant; this neglect is easily accounted for. Excepting from one or two individuals, the only remarks I ever heard in the *inspection room*, relating to the animal fluids, are a few scattered observations connected with the bare fact of the blood being found fluid in certain fatal cases.

Surely there must be some cases where the contents of the chyliferous and of the lymphatic vessels might be examined physically, chemically, and microscopically, with advantage. If the facts which I bring forward in these lectures, should have the effect of exciting a little more attention to the state of the fluids in health and disease, I shall feel satisfied that any little trouble on my part, will not have been entirely misapplied.

Boerhaave's opinions respecting the chyle and the process of digestion, were founded in part upon the experiments of Dr. Lewer. In his school it was taught that chyle prepared from bad food, or from good food not well digested, is crude in its qualities, and that this crude chyle is carried into the blood and communicates to that fluid morbid qualities, which again become the cause of fever and other diseases. It was laid down as a principle, to give mild diet in acute diseases, in order to form a sweet chyle, and thereby allay the "acrimony of the humours." Many of the details entered into by Boerhaave were purely conjectural, and are now known to be quite erroneous; but let us look to the theory, which, for so many years has cast aside, not the conjectures alone, but the facts also, and the principles which was founded upon them.

Cullen arranged dyspepsia in the class *neuroses*, thereby referring it to "a preternatural affection of sense or motion, or of those powers of the system upon which sense and motion depend," and by placing it in the order *adynamia*, it is made to consist in a weakness or loss of motion, of one of the

natural functions. All the local symptoms are regarded as depending on a loss of tone in the muscular fibres of the stomach, which is the proximate cause of the disease. Cullen himself admitted that this is not always the proximate cause, and that dyspepsia may upon occasion, depend on a change in the quantity and the quality of the digestive fluids, and I am aware that his followers have somewhat enlarged on the latter view. But Cullen regarded this as a very rare and a very unimportant circumstance; and the only *curative* indication which he lays down is "to restore the tone of the stomach," which is to be effected by local and general tonics, such as acids, aromatics, chalybeates, and bitters; admitting, at the same time, that the long-continued use of bitters destroys the tone of the stomach. Mineral waters, exercise, and the application of cold, are also employed with the same curative intention. The word chyle does not occur once, and the word blood only once, incidentally, in Cullen's chapters on dyspepsia.

And will it be said, that any material improvement is to be met with, either in the theory or the practice of the present day? Always excepting a few individuals who see further than the slavery of authority permits them to avow; and again, a few who have the hardihood to submit their labours to neglect, and themselves to contumely. I answer with deference, but at the same time with the firmness of conviction,—none. In a very comprehensive article on indigestion, in Dr. Copland's *Dictionary of Medicine*, published in 1838, the words *blood* and *chyle* occur just as frequently as they do in the *First Lines of the Practice of Physic*—once. Is it then surprising, if we meet with complaints that "the pathology of the disease is little understood," and the "method of its treatment still imperfectly known?" Or can we wonder that the reviewer of Dr. Coomb's work, in the *British and Foreign Journal*, should complain of the little attention being paid by medical practitioners to the subject of diet, and that the progress of medical knowledge has not been fertile in good dietetic principles for the treatment of indigestion?

Hunger, and the corresponding emotions by which food is obtained, with the muscular actions which prepare it, and propel it through the first passages, are functions referable to the nervous system, but digestion is mainly effected by various secretions from the blood. It is true the latter are influenced by *nervous* impressions; but it is equally true that the quantity and quality of the digestive fluids, as well as the capability of the nervous and muscular apparatus to perform their functions, depend on the *blood*. The modern acceptation of the term digestion, makes this process terminate where chyle is absorbed. Apart from the liquids absorbed from the alimentary canal, and the soluble principles which they carry immediately into the blood, the *alpha* and the *omega* of the digestive process is the formation of good chyle. Some facts in this lecture show that the formation of good chyle depends on the nature of the ingesta, as well as on the perfection of the processes to which it is submitted. The true curative indication, and that which comprehends all the rest, in cases of indigestion, is, therefore, *to promote the formation of good chyle*. The ordinary curative indication, *to restore the tone of the stomach*, has ever appeared to me as being extremely narrow in scope. Totally inadequate to convey to the mind that which it is necessary to comprise in the cure of any case of indigestion, and fertile only in partial views of the nature of the case, and an undue reliance on partial modes of treatment.

Had physicians adopted such a curative indication, I cannot help thinking that by this time physiologists would have discovered more precise means of fulfilling its intentions. As it is, by keeping steadily in view all that conduces to the formation of good chyle, and the elaborations of this chyle into good blood, we are led to the most direct means of improving the digestive secretions, and of restoring the lost muscular and nervous powers of the organs principally involved. Moreover, this indication will enable us, much better than any other, to secure the co-operation of our patient. To him the word chyle would be as unintelligible as "the sentient extremities of the nerves," and "the fibrillary contraction of the tissues;" but, if we tell him in the vulgar tongue, that the object of all our rules, restrictions, and prescriptions, is the formation of good blood, we are likely to carry his common sense with us. We can convey a much better notion of the importance of wholesome diet, good air, proper exercise, comfortable ablutions, and suitable medicines, by this explanation, than by any other. Nor is this a matter of slight importance in a philanthropical point of view. There seems to be a state on this side the grave, common to all classes in civilised society, to the inhabitants of the palace and of the workhouse; viz., indigestion. "Indigestion," says Dr. Gregory, "is the most frequent of all diseases; it is met with in every season, in every country, and in every class of society." What are the causes of all this, but the *nonnaturals* of olden time: air, food, drink, motion, rest, sleep, watching, retention, excretions, and the passions, with the quality *bad* attached to them; acting mediately or immediately upon the *blood*, and modifying its quantity, or deteriorating its vital properties. Hence dyspepsia and hypocondriacism in the affluent, and moral and physical degeneration, with its horrible train of vice and disease, in the poor. To remedy these evils, how comparatively un-

important are the drugs which come from all the regions on the face of the globe! and I put it to the profession, whether our influence is not such, that we should confer a great benefit on society by reducing the special pathology in this case to its legitimate value, both in theory and practice; and by adopting the more comprehensive views at which I have ventured to hint in this lecture.

I shall have to recur to the subject of *chyle* after we have studied the blood, and when the *lymph* is under consideration.

OBSERVATIONS

ON THE

PROPRIETY OF EXTIRPATING THE CYST

IN SOME CASES OF

OVARIAN DROPSY.

By JOHN GORHAM, *Esq.*, M.R.C.S.

THE circumstances which have induced me to make some remarks on the propriety of excising the cyst in certain cases of ovarian dropsy, are the following:—Some months since I was presented with an ovarian cyst of considerable size, capable of containing, when recent, upwards of twenty pints of fluid, which had been removed from a patient by Mr. West, of Tunbridge, Kent. This patient had been labouring under symptoms of ovarian disease for 13 years. Instead of puncturing in the usual way, Mr. West determined to extirpate. He did so, and the recovery was rapid and complete. When transcribing the cases, I shall have occasion to consider somewhat more in detail the circumstances attendant on, and the two individuals so deeply concerned in, this operation—the surgeon, as regards his great responsibility; and the patient, whose life becomes endangered; because it involves so much, is so opposed to the practice and to the rules enforced by the teachers of the present day, is so bold in its undertaking, and so apparently hazardous in its result, that it would seem almost emperical even to advocate, and rash to perform it.

The ordinary plan of treatment of ovarian dropsy, with its result, may be summed up in a few words. It appears that physicians of the present day are little disposed to differ from Dr. William Hunter, who wrote more than half a century ago, concerning the ovarian dropsy, and whose experience is stated in the following concise terms:—"I have had occasion to see a great number of encysted dropsies, many of them treated by physicians of the first rank, and yet have never seen one cured; nor have I ever known one case of that kind where the cyst has been sensibly diminished in bulk by any other means than by the trocar. If I may form a judgment from what I have seen, both in the living and dead body, I should believe that the dropsy of the ovaries is an incurable disease, and that a patient will have the best chance of living longest under it who does the least to get rid of it. The trocar is almost the only palliative."*

Very little can be added in favour of any mode which has been adopted since this period.

The remedies in use at the present day may all be conveniently classed under two heads—the topical and the constitutional.

1. *Of the Topical Treatment.*

Most prominent; all prevalent in practice; temporary as to its good results in the majority of cases; disastrous as to its effects in many, and holding out a prospect of cure to almost none, stands the operation of *paracentesis*. The relief afforded by tapping is often so sudden and entire that the patient deludes herself; she entertains the idea that this is the remedy which is to prove effectual in promoting a radical cure, and would fain hope that the case is not so bad as it may have been represented to her by her medical attendant. Yet the sac soon refills. She finds the relief afforded was but temporary, and she is compelled to submit to another tapping, and so repeatedly in some instances is this performed that the integuments of the abdomen become as it were studded with hardened masses, and even the practitioner is afraid again to perform the operation. Abernethy made the remark on a case of this sort, that "It would not do to go on boring any more holes in the patient's belly."

The relief afforded by *paracentesis* is but temporary, and yet it is almost the only palliative we possess. Of its advantages, I shall speak more at large in a subsequent part of this paper.

The next topical measure I shall notice is that proposed by Dr. Hamilton. I allude to compression and percussion. The professor's experience is comprised in the following sentences.—"That after sixteen years trial he has succeeded in a number of cases in curing or retarding the disease by the simple means above alluded to, viz., firm compression of the abdomen, percussion, the use of the warm-bath, and a protracted course of the muriate of lime, together with the ordinary means of promoting the general health. I do not know whether this plan has succeeded in the practice of other physicians, but should think if such were the case it would have been made public before this period. *Mercurial frictions* have been used, but the relief afforded is only temporary, and by some they are

* Med. Observ. and Enquiries, vol. II., p. 41.

strongly deprecated. *Extensive incisions* into the ovary have been advised, and cutting out a piece of the cyst; but the result of both these methods is unfavourable.

Another topical measure has been adopted, and it would seem with success, although it appears probable that the other means employed had a considerable share in producing a cure. This case is reported by Dr. Barnard, in which, "after the last *tapping*, a *large seton* was passed through a large space of integuments, near the right groin, and kept constantly open. A dozen leeches were weekly applied in the diseased neighbourhood for one month. The *water accumulated*, but *more slowly than usual*. She now took at night 2 grs. of opium, 3 grs. of calomel, and 3 of antimonial powder. The first night a copious perspiration set in, which abated pain, and lessened the swelling. She persevered for one month in this course, only omitting the calomel, because it made her mouth sore. As long as there was water in the sac, she was deluged in perspiration at night. At last she completely recovered. It is now more than three years since this recovery. She has enjoyed excellent health, and has frequently travelled with impunity more than 40 miles in a day."*

There are few cases where such signal success has followed a treatment which appears at first sight likely to debilitate a patient, and, unless in good hands, might prove prejudicial by diminishing the powers already depressed.

Dropsical accumulation in the other cavities of the body is for the most part a symptom not of real disease in the serous membrane which contains the fluid, but of some distant organ or organs, by which obstruction to the progression or retrogression of the various circulating fluids is brought about, or a due diminution of deleterious matter is prevented. So liver and heart-disease produce ascites and hydrothorax, and renal disease, anasarca, &c. Not so in ovarian dropsy; the fluid accumulates without a palpable cause, the symptom is at once the disease. We can, therefore, direct our remedies neither to this distant organ nor to that; and if they be severe, and calculated, as we imagine, to diminish the local disease, the constitution often becomes injured, no other effect being produced; so that practitioners of the present day trust little to remedies internally administered.

Diuretics, diaphoretics, and purgatives, whether given in large or small quantities, seem to have little effect in reducing the size of the cyst. Sufficient time has not elapsed to allow us to form any just conclusion as to the beneficial result of the administration of iodine. However, a case is related by Dr. Thomson, in which this substance appeared to have effected a cure.*

Although this case is interesting, yet it is by no means conclusive that to the iodine is to be attributed the successful termination of the case, inasmuch as the paracentesis may have produced so much irritation and inflammation as to cause the sides of the cyst to adhere, and so obliterate the cavity.

Probably we shall not diverge much from the truth in stating, that when the ovary is of considerable size, the patient's health is tolerably good, the respiration unimpeded, and circumstances on the whole allowing an abundance of time for deliberation as to the best mode of proceeding, we look in vain for any remedy which shall certainly either retard the future growth of the sac, or tend in any way to promote a cure. And, again, when the ovary is much larger, when the health of the patient suffers, when dyspnœa and other untoward symptoms present themselves, we fly to tapping as a short relief; but by this we do not cure.

Of the Treatment by Excision.

By no means a small number of surgeons have at various times proposed the operation of extirpation. It has been performed with success in this country by Lizars, Smith, King, Jeafferson, and West. Dr. William Hunter alludes to a plan which, he says, might be advisable; and as his account is concise, I shall describe it, because it not only shows that the suggestion of excision is by no means new, but also demonstrates the mode of performing the same operation as that which more recently has claimed for itself an appearance of originality. "If it be proposed, indeed, to make such a wound in the belly as will admit only two fingers or so, and then to tap the bag and draw it out, so as to bring its root or pedunale close to the wound of the belly, that the surgeon may cut it without introducing his hand, surely in a case otherwise so desperate it might be advisable to do it, could we beforehand know that the circumstances would admit of such treatment." By the word *circumstances*, Dr. Hunter, no doubt, alluded to those obstacles which will be considered in another part of this paper.

While, at the present period, then, many advocate and some perform, by far the greater number oppose even the idea of the operation. It would not be difficult to show that what has been urged against it, has consisted, for the most part, of opinion and presumptive evidence, rather than of that obtained from the accumulated experience resulting from the actual performance of the operation in even a small number of cases. However, as some of the most talented members of the profession have publicly an-

* Lancet, Jan. 9, 1829, p. 511.

* Lancet, Oct. 31, 1829.

nounced their fears as to the result, and have given, in a no less candid manner, their reasons for objecting to the operation, it is not to be expected that medical men, from the ordinary course of practice, would fly boldly in the face of danger; seeing that they had been taught to expect nothing but the most lamentable consequences from such a mode of procedure.

But cases in which the operation has been performed, begin slowly to accumulate; and far from bringing with them all that misery and fatality we had been taught to expect, the evidence concerning them is, on the other hand, comparatively satisfactory; and, to my own knowledge, three surgeons who have performed the operation in succession, and two of them more than once or twice, all advocate it; their patients not only having recovered, but, in addition, freed of their disease radically and effectually.

I think these cases are very important, and have therefore transcribed them.*

In another part of this paper, I have inserted a table, which contains a report of the most prominent features that presented themselves in ten cases subjected to the operation for the removal of the cyst.

There are, however, many, and these very serious, complications advanced as militating against the operation, and to these I purpose especially to direct attention.

The greatest hazard arises, says a late author, from the wound in the peritoneum; which must be large, if the walls of the sac be thick and solid, or if its base be broad. The fear of excising this membrane to any extent, does, no doubt, deter a vast number from undertaking the operation. Hence it will be necessary to survey impartially the effects of certain lesions that have involved the peritoneum, and particularly to inquire into the condition of this membrane before it has been touched by the scalpul; for it seems extremely probable, from the researches of Dr. Blundell, as well as from the result of some surgical operations, that a peritoneum in a previously healthy condition, will bear very rough usage. Extensive abdominal incisions are not necessarily fatal. The operations of Lizars, King, and others, abundantly prove this. Dr. Blundell, when treating of the Cæsarian section, used to say in his lectures, "When I first turned my attention to the profession, I used not unfrequently to hear that, like wildfire, an inflammation commencing in a spot of the peritoneum, might be expected to spread rapidly over its whole surface. When, however, we have not the good sense to close our eyes and ears to what is passing around us, experience, troublesome and presumptuous experience, has sometimes the insolence to contradict, without qualification, our most favourite opinions. That the risk of peritonitis, from local injuries of the peritoneum, has been exaggerated, I have endeavoured to show in a small paper printed in the physiological researches,; and from the adverse opinion of my contemporaries, I confidently appeal to posterity. In some future age, when our hearts and their petty passions are quiet in the dust, this opinion, not merely the plaything of a medical society, but, whether right or wrong, of great importance to our race in all future ages, will probably be decided by accumulative experience. May I not add, in the affirmative?"

From the collection of facts which this physiologist had been able to accumulate in the course of five or six years, the following *presumptive evidences* were drawn by himself:—

First. That smaller wounds of the peritoneum, as in tapping, hernia, &c., do not, in general, induce fatal peritonitis, or other fatal effects; and, therefore, that the common opinion, not, perhaps, formed on paper, but frequently urged in conversation, and apparently operative in practice, I mean, that inflammation in a spot of the peritoneum will almost invariably diffuse itself over the greater part of it, is probably unfounded in truth.

Secondly. That extensive divisions of the peritoneum are certainly not of necessity fatal, whether by inflammation or otherwise, and probably not generally so.

Thirdly. That the womb, spleen, and ovaries, may be taken away; certainly without of necessity destroying life, and presumptively without generally destroying it.

And generally—That the peritoneum and abdominal viscera, though very tender in the human body, will, without fatal consequences, bear more injury than, from their modes of practice, the British surgeons, especially, seem disposed to admit.

I have deemed it quite necessary to quote thus largely from Dr. Blundell, in support of a position necessary when advocating the operation, and which may be stated thus:—*The mere circumstance of making an incision into a peritoneum previously uninflamed, is not so rash a proceeding as is generally supposed.* But, it may be said, inflammation, palpable inflammation, already exists in many cases that apply for relief. This is true; but it seems pretty certain that a large number go on for a considerable period, and the ovary acquires a very large size without manifesting any sign of inflammation. Nor is it to be imagined that one constant plan is intended to be advocated. Certain cases will admit of much coarser and rougher treatment than others, and it is but fair to examine into the danger that has accrued with the apparent cause, and, if possible, to avoid

* As the first three cases of Mr. Gorham have been already published in THE LANCET, we take the liberty of omitting them.

that cause, or accumulation of causes, by selecting cases for perfect operation.

It has been said, that the wound must be large if the walls of the sac be thick and solid, or if its base broad, and the great extent of incision has been urged as an objection in certain cases. But, if extensive, it is not of necessity fatal. Mr. King made an incision eleven or twelve inches in length; his patient recovered. We have not sufficient grounds for asserting that this would be the result in a majority of cases, but I mention what has been done, in order to take a view of the subject in a practical manner, being well aware that there are many and weighty arguments on both sides. Again, those who inveigh against an incision of any kind, be it ever so small, do not hesitate to plunge a trocar into the abdomen; as if a punctured wound were not as likely, and perhaps more so, to produce inflammation and other disastrous effects; and, in truth, this does happen frequently, as may be seen by examining reports of cures with care and impartiality.

A very strong proof of the impunity with which operations, conducted with considerable roughness, may sometimes be successful, is contained in the 33rd Vol. of the Phil. Trans. by Dr. Houston; who operated, more than a century ago, on a patient at 58, who was the subject of an ovarian tumour of thirteen years duration. I subjoin the case in the doctor's words:—"The operation of puncturing the abdomen being proposed, she consented. Accordingly, with an imposthume lancet, I laid open about an inch, but, finding nothing issue, I enlarged it to two inches, and even then came nothing forth but a little thin yellowish serum; so I ventured to lay it open about two inches more. I was not a little startled, after so large an aperture, to find only a gelatinous substance bunging up the orifice. The only difficulty, however, was to remove it. I tried my probe, and endeavoured with my fingers, but all in vain. It was so slippery that it eluded every touch, and the strongest hold I could take. I wanted, in this place, almost every thing necessary, but bethought me of a very odd instrument, and yet as good in its consequences, because it answered the end proposed. I took a strong fir splinter, such as the poor use in that country to burn instead of candles; I wrapped about the end of the splinter some loose lint, and thrust it into the wound, and, by turning and winding it, I drew out about two yards in length of a substance thicker than jelly, or rather like glue fresh made and hung out to dry. Its breadth was about ten inches. This was followed by nine full quarts of such matter as is met with in steatomatous and atheromatous tumours, with several hydatids of various size, containing a yellowish serum, the least of them larger than an orange, with several large pieces of membranes, which seemed to be parts of the distended ovary. I then squeezed out all I could, and stitched up the wound in three places." The patient recovered, and lived fourteen years afterwards, without any return of the disease.

The second objection is based on the probable existence of adhesions. There can be but one opinion when extensive adhesions have united the cyst either to the parietes or to some of the adjacent viscera, for this altogether frustrates the attempt at excision. It is right, however, to observe, that when this obstacle exists in a minor degree, or even to a very considerable extent, it *may* be overcome. In Dr. N. Smith's case, adhesions covering a surface of two square inches, had glued the cyst to the peritoneum, covering the walls of the abdomen; these were torn through, and the patient recovered.

There can be no doubt that the frequency of adhesions has been greatly overrated. It is true, that in many cases, these do exist *before paracentesis*, yet in by far the greater number, it would seem that this operation has been the palpable cause of them; and it must be recollected, that in consequence of the present mode of treatment, it does not often fall to our lot to examine a body with ovarian dropsy, which has not been tapped once, or a number of times, during life. Dr. Seymour has stated, only a few months since, in a clinical lecture, which appeared in THE LANCET, that adhesions exist in 99 cases out of 100. This is surely wide of the true state of the matter, unless the Doctor, in forming this average, excluded all cases in which the operation had not been had recourse to. In Mr. Jeafferson's case, Mr. King's, and Mr. West's, adhesion had not taken place. From the first, twelve pints of fluid had been extracted; from the second, twenty-seven; and from the third, more than twenty; so that in each the tumour had proceeded to a considerable extent; but neither of these patients, be it observed, had been tapped. It would not be difficult to multiply instances where extensive ovarian sacs have existed without any adhesion whatever, even after paracentesis has been performed. Dr. N. Smith states he has had an opportunity of dissecting a body of a patient who had died of an ovarian dropsy, after having been tapped seven times. In this case the sac was found to be the right ovarium, which filled the whole abdomen, but adhered to no part except the proper ligament, which was not larger than the finger. He also mentions that he has seen two other ovarian sacs, which were taken from patients after death. They had been tapped several times, and the sacs were equally unattached, except to their own ligaments. Hence he inferred, that in a case of ovarian dropsy, while the tumour remained moveable, it might be removed with a prospect of success, and the event, he adds, has justified the expectation. Mr. Jeafferson also met with a dissection, which

also verified the opinion that the disease was sometimes simply saccular and inadherent, and that this sac might, after the escape of the fluid, be readily extracted through a small opening. It must be stated, however, that in almost all the cases of ovarian dropsy reported by Dr. Bright, in his late communication on abdominal tumours, recourse had been had to tapping, and adhesions excited anteriorly, that is to say, the peritoneum lining the parietes of the abdomen was glued for a greater or less extent to the cyst, as it would seem, from inflammation produced primarily by the puncture. From what has been said, the following appears to be a tolerably lawful inference:—The existence of adhesion is by no means an unfrequent complication of ovarian dropsy, the cyst being glued to some viscus in a few instances—more frequently, and with greater certainty, to the walls of the abdomen; the cause of this latter being in most instances referrable to inflammation set up by the process of tapping.

As to the diagnosis of adhesion, a peculiar feel, a sort of cripitus, derived from pressing the hand on the parts where the adhesion has taken place, will in some cases be instructive; add to this a large tumour of some considerable standing, the circumstance of a previous paracentesis, or several, and the existence of adhesion results. On the other hand, we have strong presumptive evidence, in favour of the supposition that when an ovarian tumour of tolerably recent date exists; when it is sometimes to be ascertained by examination *per vaginam*, and can be pushed with the finger to another part of the abdomen; when it can be moved with the hand placed on the surface of the abdomen, and when *paracentesis* has never been performed, that it is not adherent. There is, however, no certain diagnostic sign to guide us on this head. But I will suppose adhesions to exist, and these extensive and firm. I will suppose an incision to have been made into the abdomen, of some inches in extent, say two or three, or even a dozen—does the patient under such circumstances necessarily die? This question must be answered in the negative. Presumptive evidence is afforded in support of this by Dr. Blundell, and some of the cases contained in this paper tend further to prove it.

On the whole then, and to sum up, we may state that, extensive adhesion, as promulgated by those who oppose the operation of excision, is certainly an important obstacle, cases as treated in the present day, abundantly proving its very frequent existence; and yet that there is good reason to believe that if *paracentesis* were less frequently performed, adhesions would consequently be less frequent; and, moreover, that exploring the abdomen in either case is not so dangerous and hazardous as we are taught to believe; and lastly, that in some cases, we can make ourselves aware, with a degree of certainty, either of the presence or absence of adhesions, and so direct our measures accordingly.

A third objection is stated to be, the existence of numerous cysts. This is a very serious objection indeed, in the case of adhesion having formed; if otherwise, this third objection merges, as will be seen, into the second, the presence or absence of adhesion forming the peculiarity in each.

Again, it is urged against incision, that patients may live many years without any interference at all. This is well known, but I would remark, from Dr. Bright's experience, it would appear that cases which continue above four years from the first necessity for the operation of *paracentesis*, bear a small proportion to those which prove fatal before that time. Let the early period once pass by; let inflammation occur; let the operation of *paracentesis* be performed but once, and the chances of actually curing are all reduced to a mere conjecture, I might almost say, to a forlorn hope. Hence a rule: *If excision be performed at all let it be done early.* Dr. Hamilton objects to the operation in the following terms: "There is difficulty of diagnosis in the early stage, therefore the operation is useless or unnecessary; useless, if there be no disease, and unnecessary, if the disease be stationary." The first proposition requires no comment, as every one knows that it would be of little service to operate on a sound patient. Dr. Hamilton, I am aware, here alludes to the difficulty of diagnosis; the question hinges on this, and it cannot affect the operation. If diagnosis were more exact the operation would be more useful, as regards the second statement, viz. that the operation is unnecessary if the disease be stationary. I may remark that this would appear to be the period above all others when an operation would be most likely to be attended with a good result; besides, a large number of patients are anxious to be freed of morbid growths, almost at any peril, more particularly of those which have gained the appellation of tumour, and the constant irritation produced on the mind, is, in some cases, as wearing and trying to the constitution as the ravages of actual corporeal and organic disease.

Lastly, considering how many very serious objections may be urged against *paracentesis*; why this should be in such repute, to the almost total extinction of the mode by incision, I know not. As to the relief afforded by tapping, it is more generally only temporary. "There are," says Dr. Bright, "I believe, a few instances on record, where this operation (*paracentesis*) has apparently been followed by complete cure." This is saying very little in its favour, considering the many thousands of times it has been performed. There are, too, several objections

against it, which may be enumerated concisely as follows:—1st. Exhaustion may follow the too rapid evacuation of the fluid. 2nd. Inflammation of the peritoneum may supervene, and thus life be destroyed. 3rd. The sac refills, and requires repeated tapping. 4th. There may be a many-celled encysted dropsy, or a too viscid fluid; or, 5th. The patient may sink without any evident cause. Scarcely more can be urged; and, to me, it seems not half so much against the other, or excision plan. Instead of thousands, few operations have as yet been performed, and these have succeeded beyond expectation. As regards the rapid evacuation of fluid, the danger is as great in the one case as in the other. As to subsequent inflammation, this occurs in a less ratio after extirpation, on actual comparison; no sac is left to refill, and through the incision in the abdomen we are enabled to take away a sac of considerable thickness, with many cells, whether the fluid be viscid or not.

Finally, I may remark, by quoting from Dr. Seymour, that "in the lower animals, while unimpregnated, the extraction of the ovary is attended with little or no danger. It is said also, that both in ancient and modern times, the extirpation has taken place to gratify the cruel and barbarous profligacy of Asiatic monarchs."

Tunbridge, Kent, Oct. 10, 1839.

P.S.—I may add, in conclusion, that Mr. West has himself perused this paper, and has allowed me to insert his name, which gives an additional weight to the sentiments that are but imperfectly set forth throughout its pages. I have the more gladly availed myself of this, because the major part of the cases contained below, were given to me by Mr. West, who has had to contend with the difficulties, not only of the operation itself, but also the diagnosis, and the selection of appropriate cases, where it might be done with the greatest chance of success.

The following condensed report of the cases in which the operation was performed, contains the undermentioned particulars relating to each case:—

The names of the operator and the patient; the patient's age; the symptoms which indicated the propriety of operating; the symptoms during, or after, the operation; a statement if before tapped, and if adhesions existed; the after-treatment; and the results. In each case, excepting the last, the operation was the same as is described and advocated in the above paper:—

I. Mr. Jeafferson (Lancet, Jan. 7, 1837).—Mrs. B., aged between 30 and 40.—Ovarian tumour; integuments thin; fluctuation vivid; tumour interfered in some little degree with the performance of her domestic duties. 12 pints of fluid extracted. No pressing disturbance during the operation. Second day, vomiting, hiccup, low pulse, and griping pain in the abdomen; no more unpleasant symptoms. Digitalis, opium, and cold wet cloths prescribed. Perfectly cured, and able to perform all the duties of her station.

II. Mr. King (Lancet, Jan. 21, 1837).—Hannah Cavell, aged 37.—Enlargement for three years, and increasing; health tolerable; not much pain; emaciation; the enlargement owing to ovarian disease. 27 pints of fluid escaped. Next day slight tenderness; pulse 64; she came down stairs in a week after the operation. Treatment after the operation the same as in case I. Perfectly cured.

III. Mr. West (Lancet, Nov. 25, 1837).—Mrs. Hurrion, aged 45.—Tumour for 13 years; evidently ovarian; fluctuating and now impeding respiration. Upwards of 20 pints of fluid withdrawn. Day after the operation, little pain; pulse, 90; on the following day more pain; pulse 100; sickness; bowels confined. Calomel, opium, cold spirit lotions, saline aperients, and enemata employed. Perfectly cured, recovery being rapid. The cyst is in the museum of Guy's Hospital.

IV. Mr. West (unpublished).—Miss S., aged 23.—Obstruction of urine and fœces. These symptoms gradually vanished, and a tumour appeared, ascending into the abdomen, which was ovarian, and in which the fluctuation was particularly distinct. 24 pints of fluid escaped. No unfavourable symptom after the operation. The same treatment as in case III. Cured.

V. Mr. West (unpublished).—Mrs. Tomkins, aged 40.—Ovarian tumour. The patient had previously been tapped. Adhesions existed. She recovered, not cured; she has been tapped 17 times since.

VI. Mr. Smith (*Edinburgh Medical and Surgical Journal*, p. 532, Oct.)—Mrs. Trowbridge, aged 33.—Large ovarian tumour on right side; fluctuation very distinct. 8 pints withdrawn. No unfavourable symptom after the operation. There were adhesions to the extent of two square inches to the parietes. Plaister and bandage used. Perfect recovery—cured.

VII. Mr. West (unpublished).—A. M., aged 24.—Same symptoms as in case VI.; the sac contained 11 gallons, and her constitution was much shattered. 8 pints withdrawn. Tapped repeatedly before. She sank.

VIII. Mr. King (Lancet, loco citato).—Sophia Puttock, aged 40.—Tumour supposed to be ovarian, and fluctuating. After an unsuccessful exploration no tumour was found. She recovered.

IX. Mr. Hargraves.—A. B., aged about 40.—Same symptoms as in case VIII.; fluctuation distinct, but a sense of solid tumour in many parts of the abdomen. Especial care was taken not to use in the

operation any but the *slightest* friction in attempting to procure the removal of the cyst. Some pain for an hour or two after the operation. Adhesions, and many cysts, with thickened walls, existed. Opium, and bandage, employed. Perfect recovery; not cured; she was sitting up and dressed on the fourth day after the operation.—[Mr. Hargraves kindly invited me to witness this operation, and has allowed me to insert this report of it.]

X. Operation performed in Guy's Hospital in 1839.—Ovarian tumour; fluctuation distinct; health tolerable. The operation in this case was not the same as that advocated in the foregoing paper, more traction being made at the cyst, and more internal exploration. There was slight adhesion. She sank. [The cyst was not removed in this case. The post mortem examination seemed to throw much light as to the cause; for it appeared, on laying open the abdomen, that the first cyst was in connection with a second, of nearly, if not quite, the same magnitude. The cavities of the two were not, however, in communication with each other. Probably, if this non-punctured cyst had been discovered, by making the incision a little longer, a less degree of traction would have been used, and with a more favourable result.]

GENERAL INFIRMARY, HERTFORD.

FRACTURE OF THE BASE OF THE SKULL—LESION OF THE SEVENTH NERVE, FOLLOWED BY PARALYSIS OF ONE SIDE OF THE FACE—SUICIDE FIVE MONTHS AFTER THE INJURY—AUTOPSY.

By JOHN DAVIES, *Esq., Surgeon.*

JOS. BYNOTH, aged 31, a tall, fine-looking man, was brought into the General Infirmary, under my care, on the 1st May last, 1839. When brought in he was perfectly insensible, and bleeding copiously from the ears, nose, and mouth. The account given of the accident was, that the man was either standing or sitting on the shaft of a cart loaded with gravel, when the cart tilted back and pitched him over on his head, which rendered him at once insensible. Soon after his admission his sensibility began to return, and, under the usual antiphlogistic plan of treatment, he recovered so far that, at his own request, he was discharged on the 9th of the same month. In fact, no *serious* symptom showed itself after the return of sensibility.

The effects of the accident were the following:—Perfect insensibility for about an hour, during which time the bleeding from the nose and ears, particularly the left, was very copious; then a gradual return of sensibility, and cessation of bleeding; no fracture could be discovered in any part of the skull, though it was suspected that its base was fractured; the mouth drawn slightly towards the right side; deafness of the left ear; a sort of singing or hissing noise in the head; these were the only symptoms of cerebral lesion discovered while the man remained in the hospital. The normal signs were—absence of all delirium and of mental disturbance; pupils of the eyes natural; pulse moderate, and non-occurrence of any febrile symptoms; sleep natural; in a word, the absence, up to the time of his leaving the infirmary, of any symptoms indicating severe lesion of the brain.

When I visited the patient a few days after he had left the infirmary, I noted the following symptoms, which were not all discovered whilst he was lying in bed at the hospital:—The mouth was drawn considerably to the *right* side; total absence of the power of *motion* of the *left* side of the face; sense of *feeling* equally natural on both sides; an attempt at smiling caused a very ridiculous appearance of the countenance; it gave you the idea of a person having two faces, one smiling at you, while the other was staring at you with the most profound gravity; the right side of the forehead presented the natural wrinkling of the skin, while the left looked quite smooth, and no effort could induce it to change its appearance; the left *ala* of the nose fell in upon the *septum narium*, and the man said that he was occasionally obliged to take hold of that side of the nose and pull it open; in eating and drinking he was conscious that he possessed no power over the left side of the face, because when the food got between the cheek and the gums, he was obliged to introduce his finger to scoop it out, and he could not drink, he said, without closing the left side of his mouth with his hand, or by applying the cup to that side, and inclining his head to the right, for the fluid ran out at the left angle of the mouth; he could not close his left eye; no effort could enable him to make the eye-lids approach each other. When told to shut his eyes, the right one was immediately closed, and the pupil of the left was at the same instant whipped up under the upper lid, but leaving the lids perfectly open, so as to expose the white of the eye; he was not at all conscious that the left eye did not shut at the same time as the right, but he said "he thought there must be something wrong about his eyes, because his wife told him that he never slept;" no portion of the cornea of the left eye was visible when the right was closed; and, according to his account, the one was in total darkness equal to the other; he felt no inconvenience from the inability to close the eye-lids, for the eye was not at all inflamed.

All the above symptoms continued the same, and of the same degree, up to the time

of the man's death, which took place, as shall be immediately described, just five months after the accident. I had several opportunities of noticing them, and of causing the experiments on the eye to be repeated. The noise in the head, with a slight discharge from the left ear, lasted for about four months; but what is rather remarkable is, that, according to his own account, the hearing of the *left* ear, which was at first destroyed by the accident, returned after about a month, and that of the *right* ear became extinct, or considerably diminished. During the last three months, he frequently, in my presence, applied his watch to his ears, and declared that he could hear it tick very well with his *left*, but that he could not hear it with his *right*.

The man remained for about four months without returning to his work, but he worked at the harvest, &c., during the last four or five weeks.

The opinion which I entertained of the case, and which I expressed to several medical friends, was, that the basis of the skull had been fractured by the accident, which was rendered probable by the copious hæmorrhage from the ears and nose, though no grave symptoms of cerebral derangement occurred after the concussion caused by the fall; that the fracture had either occasioned a rupture of the *portio dura* of the seventh cerebral nerve, or produced a permanent pressure upon some part of it in its passage through the bone; or else that extravasated blood pressed upon some part of its course within the cranium. The symptoms, after the first effect of the concussion produced by the fall, were exactly those so graphically described by Sir Charles Bell, as proceeding from destruction of the function of the *portio dura* of the seventh cerebral nerve.

In the night of the 30th Sept. last, Bynoth, having suffered for three or four days from the prevailing bowel complaint, exhibited, all of a sudden, symptoms of delirium, occasioned in a measure, it was supposed, by some unpleasant family affairs, committed suicide, by cutting his throat with a razor. He lived about thirty hours after committing the act. I had seen him in the morning of the same day: he then appeared much as usual.

On post mortem examination, at which I was assisted by my friend Mr. Shillitoe, of this town, the bones of the cranium were found unusually thin; all the membranes of the brain appeared perfectly healthy, with the exception of a slight turbidity of that portion of the arachnoid situated in the left middle fossa. The brain itself was remarkably bloodless, of the natural consistence, and presenting all the appearance of a healthy brain. In the left temporal bone there were the remains of a fracture, running obliquely backwards and outwards, between the squamous and petrous portions, splitting the Eustachian canal as far as the tympanum; then continuing on, partly separating the squamous portion of the temporal from the parietal bone. At the root of the petrous portion, there was a transverse branch of the fracture, extending across from the tympanum to the groove of the lateral sinus; thus dividing the Fallopian canal, along which the portio dura of the nerve of the seventh pair finds its passage out of the skull. The fracture was so slightly united, that the mere vibration caused by sawing the bone, occasioned it to separate. This circumstance was rather singular, considering that five months had elapsed since the accident; but, in fact, all the bones of the cranium, besides being very thin, appeared more brittle than usual. The lateral sinus had not been injured, or, if it had, no indication of injury remained observable after death.

The points most remarkable in this case are—1st., that so extensive an injury should have caused such slight cerebral derangement; 2nd, that the case affords an additional illustration of the correctness of Sir Charles Bell's views, respecting the function of the nerve of the seventh pair; 3rd, the loss of hearing, first of the ear on the side of the injury, afterwards of the ear of the opposite side, but with a return of hearing on the side originally affected. The loss of hearing on the side of the injury might have been occasioned by extravasated blood blocking up the passage, so as to prevent the sound from reaching the auditory nerve, and its restoration, have taken place from the absorption or discharge of the obstacle; but it is not so easy to account for the fact that deafness occurred in the opposite ear a month after the accident.

Fractures of the base of the skull are generally considered fatal; but I see no reason why they should necessarily prove so, as *simple* fractures, any more than fractures of any other part of the cranium. It is here that the violence which produces a mere fissure in the base of the skull may also occasion a rupture of the lateral sinus, or of a considerable branch of the cerebral arteries, or cause some other severe lesion of the contents of the cranium, so as to lead to a fatal result; but so may a mere fissure in the region of the principal meningeal artery cause a rupture of that vessel, and thereby occasion death, upon the same principle; namely, by producing pressure upon the brain.

Judging from symptoms, I have reason to believe that I have witnessed three or four cases of fracture of the base of the skull, where the patients recovered, but none of them with so little cerebral disturbance as the subject of this paper.

Hertford, Oct. 10, 1839.

TWINS. — PROFUSE BLOODING. — PLUGGING AND COMPRESSION. —RECOVERY.

To the Editor of THE LANCET.

SIR:—I was called to attend Mrs. Hall, who was in labour, on Tuesday evening, Sept. 16, 1836, at 10 o'clock, p.m.; I found the labour far advanced, the soft parts dilated and dilatable, but the pains very feeble. Suddenly they became stronger, and with one tremendous pain the whole of the child was born. Notwithstanding the infant was very large, I found, on placing my hand over the abdomen, that it was very little decreased in size. On making an examination, per vaginam, I found a second bag of waters, and with great difficulty ascertained (the child being very high up) that the head presented. The pains for a time left the patient; at length they returned, and about an hour after the first child, the second was born—the largest twin I have ever seen. The first child was born at about half-past 11, and the second at about half-past 12. During the labour, and after the birth of the children, very firm pressure was applied over the abdomen, but yet the uterus did not contract, nor would the placentæ come away. After waiting about an hour, and using the usual means to cause their expulsion, and finding hæmorrhage to a fearful extent coming on, I immediately proceeded to remove them. On introducing my hand and arm into the uterus, I was obliged to carry it to the extent of my elbow, and even then I could scarcely reach the top part of the uterus. The uterus was so relaxed that I do not think it had at all contracted since the birth of the children; the children, too, be it remembered, were immensely large. With great difficulty I found the sides of the uterus, and ascertained that the placentæ were very extensively adherent to the uterus, and with still greater difficulty I separated the adhesions; all the while the blood pouring out like a torrent! After the removal of the placentæ, the uterus partially contracted, but still the flooding continued, and the life of my patient was fast, very fast, ebbing away. I lost no time then in plugging, and this I did most effectually by means of a silk handkerchief, so that she could not lose one drop of blood. Still the uterus had not properly contracted, although the firmest pressure had been applied, and I had given her a tea-spoonful and a half (at two different times) of laudanum. I therefore applied *very liberally* cold vinegar and water to the abdomen and labiæ. The patient's appearance was still deathlike; the skin was cold, and the pulse was fluttering. Brandy, slightly diluted with water, was liberally and frequently given to her. At this stage of the case, I thought it would be well to call in further assistance; I therefore sent for my friend Mr. Knowles, who most promptly and kindly attended the call. When he arrived, the patient was a little better. We thought it advisable to apply compresses of stockings and napkins over the region of the uterus, and a roller firmly over all. The patient continued in a precarious state for some hours. I saw her the next morning, and found her much better; I removed the handkerchief from the vagina; she made water, and then felt very comfortable. Suffice it to say that our patient rapidly recovered from the effects of her fearful hæmorrhage.

I am, Sir, your obedient servant,

PYE HENRY CHAVASSE, Surgeon.

Birmingham, 12, Old Square,
Oct. 9, 1839.

BLACK PITCH IN HÆMORRHOIDAL AFFECTIONS.

To the Editor of THE LANCET.

SIR:—I have been induced to forward the present communication for insertion in your valuable columns, on account of the happy results that have followed the administration of pix nigra in hæmorrhoidal affections.

Pix nigra, or what is more commonly called black pitch, has not been employed as a therapeutic remedy, so far as I am aware, in any internal complaint, either by ancient or modern writers. I was led to adopt its employment from the following circumstances:—A young female, whom I attended in the confinement of her first child, was, in one month after delivery, attacked with symptoms of internal hæmorrhoids. The best-directed medical treatment, which was continued for two months, failed to produce permanent relief. A female acquaintance who called to see her, on inquiring into the state of her health, and finding her suffering from hæmorrhoids, told her to take two small pills, composed of pix nigra. Having found no relief from medical art, she at once embraced the proposal of her friend, and took two pills. According to her own account, the tenesmus and the sinking at the pit of the stomach, almost immediately fled. When I next saw her she was much improved in her general appearance; when she informed me that pitch pills had entirely removed her complaint.

Two of the first authorities of the present day, Sir B. Brodie* and Dr. Copland,† when speaking of hæmorrhoidal affections, strongly recommend the class of medicines termed *balsams*. I may here, with equal propriety, place that medicinal agent, pix nigra, in juxta-position with the balsams. Respecting the *modus operandi*, I can add no-

* Med. Chir. Rev.
† Dic. P. Med., Art Hæmorrhoids.

thing. It is a safe and effectual remedy in internal and external hæmorrhoids, whether of a blind or bleeding character. I could adduce many more cases in support of its utility in that distressing affection. My formula is the following: pitch 3½, divided into three pills, two to be taken *every* night, this being generally sufficient to cure the complaint, attention at the same time being paid to the state of the intestinal canal.

I hope one of your numerous correspondents will enter into some investigations respecting the *modus operandi* of pix nigra, and publish the result of his researches.—I remain, sir, yours respectfully,

T. H. WARDLEWORTH.

Rochdale, October 10, 1839.

P.S. Since writing the above I have had some conversation with Mr. Dunlop, surgeon, of this place, wherein he stated that he had been in the habit of employing this remedy for the last 20 or 30 years, with the best effects, in hæmorrhoids.

TRAUMATIC TRISMUS.

To the Editor of THE LANCET.

SIR:—If you think the following case sufficiently interesting to be admitted into your valuable Journal, I will thank you to insert it. I am, Sir, your obedient servant,

ARTHUR ADYE.

Bedford, Wilts, Oct. 6, 1839.

July 12. Abraham Paradice, aged fourteen years, had his left arm drawn in between the cogs of two iron cylinders of a cloth-machine. The integuments and tendons of the lower third of the ulna were very much bruised and lacerated. There was scarcely any hæmorrhage. Having dressed the wound, the boy was taken to the Union Workhouse. He appeared to suffer acute pain in the injured part, and the wound looked sloughy and unhealthy up to the 21st of July, when, on visiting him, I observed that the nurse had wrapped his throat up in flannel, thinking he had taken cold.

On examination I found him suffering from trismus; he had violent spasms of the muscles of the affected arm and of the neck. The jaw was so firmly closed as scarcely to admit the handle of a spoon; the countenance was exceedingly distressed; the circulation hurried; the breathing anxious; and the whole surface of the body was covered with profuse perspiration, which never ceased during the continuance of the spasm.

To have a warm bath directly, and to be repeated in twelve hours; to continue poultices to the wound; and the following powders to be taken every four hours, which were given for ten days without intermission:—

Musk, two grains;
Calomel, two grains;
Comp. ipec. powder, four grains.

His mouth became affected with the calomel on the fifth day. He had port wine and broth frequently.

The boy did not appear to receive much benefit from the treatment, even after ptyalism had taken place, until the 2nd of Aug., when it was evident that the spasm was less rigid. He continued to improve till the 10th August, when it entirely left him. The wound looked much more healthy after the commencement of the trismus, and is now quite healed, and the boy is in perfect health.

EXCISION OF THE LOWER JAW AT ARMAGH.

To the Editor of THE LANCET.

SIR: In the "Students' Number" of THE LANCET, dated September 28th, 1839, I find the following remarks, preceding a case of excision of a part of the lower jaw, by Dr. Bull, of the South Infirmary, Cork:—"We believe that this is the first example of excision of the inferior maxilla having been performed by a provincial surgeon in Ireland." With reference to this remark, I must request that you will have the goodness to insert the following extract from the *Register of the Armagh County Hospital:*—"Jane Stewart, æt. 64, recommended by the Rev. Dr. Stewart, Loughjelly, town land, Ballydogherty, property of Mr. M'Cartney. Disease, schirrous tumour of the lower jaw. Admitted March 24, 1832—*half of lower jaw and tumour removed.* Discharged *cured*, April 24th, A.D. 1832. Number of days in hospital, 31," &c.

This you will perceive, sir, is upwards of seven years previous to Dr. Bull's operation, and was performed by Dr. Robinson, surgeon to the Armagh County Infirmary, assisted by me. The operation was performed pretty much in the same manner as that at the Cork Infirmary. The arteries requiring ligature were two or three at the most, and the woman was discharged with little or no deformity, capable of eating, drinking, and speaking with ease and freedom. The only incident in the operation requiring notice was the breaking of a very expensive and apparently finely fabricated chain-saw, which was immediately remedied by substituting the common amputating saw, with which both the divisions of the inferior maxilla, the one at or beyond the symphisis, and the other at about midway in the ramus, were effected. The dry preparation is preserved at the infirmary. The tumour involved both sides of the jaw-bone, and the alveoli of several molar teeth.—I am, sir, your most obedient servant,

JOHN COLVAN, M.D., M.R.C.S.I.

Armagh, Oct. 17th, 1839.

BANDAGE IN PARACENTESIS ABDOMINIS.

To the Editor of THE LANCET.

SIR:—I take leave, through the medium of your widely-circulated pages, to recommend the adoption of the eighteen-tailed bandages, in paracentesis of the peritoneal and ovarian caustics, instead of that old, clumsy, and unscientific apparatus, "the sheet," as affording, at once, great support and comfort to the sufferer, and assistance to the operator, by a regular and steady pressure, in consequence of its presenting an uninterrupted broad and flat surface over the whole of the parietes, during the operation, and, as it supersedes the usual after bandage, much fatigue and probable risk to the patient are avoided.

The bandage is made in five minutes, of stout calico, doubled, two yards long, and in width extending from the scrobiculus cordis to the pubes, the 'tails' about four inches broad, and divided to the normal size of the abdomen, so that when the fluid is drawn off, the points of their junction shall embrace the parietes sufficiently to afford the requisite support, the 'tails' are then to be passed round, and secured.

N.B. The broad part is to be applied in front, and the tails interlaced behind, and approximated by one or two assistants, during the operation.—I am, sir, your obedient servant,

T. W. WANSBROUGH.

Chelsea, Oct. 2, 1839.

IDENTITY OF COW-POX AND SMALL-POX.

To the Editor of THE LANCET.

SIR:—During the last three years, whenever an opportunity presented itself, I have tried to establish the fact that the pox, which occurs spontaneously on the cow, is the same disease as that which occurs to the human subject, and which we denominate variola or small-pox. The means which I have taken to arrive at this conclusion, are exceedingly simple, and may have been tried by others before me; but, if so, I do not think they have been published.

Having a patient, labouring under variola, I took some virus therefrom, and innoculated a cow's udder. On the sixth day, I took some lymph from the cow, and vaccinated a child, two years of age, therewith. On the third day this produced the vaccine vesicle, and at the fifth day came to maturity. I have frequently tried the experiment since, with the like success. The characteristics generally noticed, when a child is vaccinated with lymph procured in this manner, are—the vesicle is sooner formed, sooner comes to perfection, and there is greater irritation and inflammation around the pustule. These peculiarities gradually diminish, according to the number of individuals through whom the virus pours afterwards; and hence I consider the more recent the matter is taken from the cow, the greater security will there be against the individual vaccinated having the small-pox in its more malignant form, if he should happen to have it all.

I was led to perform these experiments from having seen the cow-pox go through all its proper stages in a person, who was vaccinated two days previous to the small-pox making its appearance on him; and imagining that two dissimilar eruptive diseases could not happen in the same person at once, I considered that the pox as it exists in the cow was the same disease as the small-pox in a milder form.—I am, sir, your obedient servant,

HENRY H. RUGG, Surgeon, &c.

London, Oct. 21, 1839.

TESTS OF POISON.

RECENT INQUEST AT HERTFORD.

To the Editor of THE LANCET.

SIR:—In your last Number but one, under the head "FORENSIC MEDICINE," there was (abstracted from the *Hertford Reformer*) an account of an inquest held in this town upon the body of a woman who was supposed to have been poisoned by oxalic acid, and in whose case Mr. Shillitoe and myself had been engaged in post mortem examination, and in testing the contents of the stomach. You were kind enough to transfer our report of the case at full length into THE LANCET. In that report we stated, that "In *one* respect only did the contents of the stomach fail to give the same result as a solution of oxalic acid in water. A white, heavy precipitate, as before stated, will be caused by the action of nitrate of silver on solutions of various substances; but it is characteristic of that produced on a solution of oxalic acid to *detonate*, when dried on a filter, and exposed on a spatula to the heat of a spirit lamp. No detonation could be caused by the precipitate from the liquid found in the stomach." We accounted for this absence of detonating property, by the facts that the quantity of acid in the stomach after death was very small, owing to the constant vomiting which had continued for several hours before death, and that this small quantity was mixed with a large proportion of animal matter, precipitated by the nitrate of silver, together with the acid.

Now, in a case like this, where any doubt might exist, it is satisfactory to know that a boy, living in the same house as the deceased, has, since the inquest, come forward to confess "that he bought two penny worth of oxalic acid for the deceased, a few days before her

death, at the shop of a chemist in this town; that he was charged not to mention the circumstance to her husband; and that, if he should forget the name of the article, he was to tell the chemist that it was what they clean boot-tops with."

My object in requesting you to insert this letter is to show that, in processes like that of testing the contents of the stomach in cases of suspected poisoning, one may occasionally fail to find *all* the results which he is taught to expect, or *all* the results furnished by the same tests on a simple solution of the same poison in water. Your's obediently,

JOHN DAVIES.

Hertford, Oct. 6th, 1839.

NULLITY OF THE ACT OF 1815.

To the Editor of THE LANCET.

SIR:—As the editor of an influential medical periodical, you appear to be the fittest person to bring before the public the present disgraceful state of the law, by which their worships, of Apothecaries' Hall, are governed. After having paid the customary premium to a surgeon-apothecary for my son, I, in due time, entered him at one of the medical schools and hospitals of this metropolis, ignorantly supposing, that, should he pass the examination of their learned worships, *they* would, at least, by their diploma, not only qualify him to act as an apothecary, but protect him from all others not so licensed; but how was I disappointed to find that any chemist, druggist, or even quack, has the power of taking away the advantages which ought to follow an expensive and scientific education; for such, Mr. Editor, is the course of studies enjoined for a medical student, that a small fortune may, without great care and economy, be expended before all the lectures, &c., are gone through. I would, therefore, take the liberty of suggesting the absolute necessity of forming a medical students' association, the plan for which there is no one more capable of concocting than yourself.

I have had opportunities of conversing with many students, and have found a pretty general desire prevailing amongst them, of setting the Court of Examiners, at the Hall, at defiance, and merely passing at the College of Surgeons, unless their worships take steps to enlarge their power by applying to the legislature.

PATER.

WESTMINSTER MEDICAL SOCIETY.

Saturday, October 19th, 1839.

MR. HALE THOMSON, President.

EFFECTS OF EXCESSIVE INTOXICATION BY ALCOHOL.

The first meeting of the session was held this evening in the Society's New Rooms, Exeter Hall. The well-founded objections which existed to the Hunterian Museum, as a place of meeting, prevented many of the old members of the Society from attending at that place. It was cold and cheerless on a winter's night, and sufficiently dreary in appearance to prevent any but the stauchest friends of the Society from attending. The meeting-room in Exeter Hall, on the contrary, possesses an air of comfort, and is so well warmed and lighted, that no such objections can be raised against it. We trust the present session will be more profitable than has been usual, and that the discussions will be less desultory than they have been during some former sessions. The Westminster Medical Society numbers about 1,200 members, among whom will be found the names of many distinguished men. With a little industry, its proceedings will bear comparison with those of any similar institution. The evening, as is customary on the the first meeting of a new session, was partially occupied by the nomination of members to fill the various offices.

EFFECTS OF ALCOHOL.

DR. GOLDING BIRD stated the following case:—About five months since a man applied to the Finsbury Dispensary as a patient. The apothecary thinking, from his symptoms and appearance, that the man was intoxicated, gave him an emetic and purgative, and ordered him to attend again. Dr. Bird saw him on the next visiting-day, which was three days after his first appearance. He was a fine, healthy-looking man, and, until within the last six weeks, had always enjoyed excellent health. He was employed in a distillery, and was remarkably temperate. He gave the following account of his case. Six weeks before, one day, feeling very thirsty, he went to drink out of a can, containing, as he supposed, cold water, but which turned out to be strong alcohol 50° above proof; before he found out his mistake he had drank about eight fluid ounces, and instantly dropped senseless upon the floor. A medical man saw him a few minutes afterwards, and found him deadly cold; his breathing was calm, his pulse scarcely perceptible, and his lips blue. He was taken home. His wife refused to allow the medical man to empty the stomach, but gave her husband some purgative pills, which she had great difficulty, however, in getting him to swallow. He continued in this cold insensible state for eleven hours,

at the expiration of which time he opened his eyes, stared around him, and inquired how he got home from his work. During the eleven hours he passed no urine or fæces, but, on gaining sensibility, the bowels were moved, the evacuation being of a pitch-like consistence and colour. He went to work on the following morning, and remained well for about three weeks. It was then noticed that his countenance had an unusual appearance; that he was heavy, and fell asleep over his work; and that he wanted the proper appreciation of objects, mistaking one thing for another. In this state he remained for three weeks, and then applied to the dispensary. After the operation of the emetic and purgative given to him by the apothecary, Dr. Bird found him with his pupils widely dilated, and contracting slowly under the influence of a strong light; his appearance was healthy, but he was rather pale; pulse 120; his appetite good; he was drowsy, and blundered in his work; when questioned, he was about half a minute before he answered. There was no paralysis. He was quite unfit to follow his occupation. Dr. Bird thought the case bore some analogy to delirium tremens, and after purgation, ordered him ammonia and good living; but there has been no appreciable improvement. He is in the same state at the present period as when he first came to the dispensary. He has no strabismus, or any other symptom indicating effusion, except dullness of the intellect and a heaviness of the occiput, which latter is relieved by purgatives. The motions are still of a pitch-like consistence. The points of interest in the case, Dr. Bird considered, were—1. The sudden effect of the alcohol.—2. The man becoming perfectly well at the expiration of eleven hours, remaining so for three weeks, then beginning to decline in health, and remaining very unwell up to the present time.

Dr. Chowne said that the two points of interest in the case were, the sudden effect of the liquor, and the peculiar state of the patient at the present time. In reference to the first, he believed that several cases were recorded in which such an effect had been noted. He had related a similar case in this respect at the last meeting of the Society.* It was not necessary that the quantity of spirit should be very large for this effect to be produced. Two men drank an equal quantity of spirituous liquor; in one the effects were slowly developed; in the other insensibility was instantly produced, and lasted for a considerable period. The symptoms enumerated by Dr. Bird corresponded with those usually present in these cases. It was not uncommon to observe the breathing perfectly quiet, even to the very last, in those who died from the effects of alcohol. The pulse was small and feeble, and the extremities cold. In Dr. Bird's case he was inclined to believe that the cerebral affection was more functional than organic, and might depend upon the state of the bowels; this opinion was also corroborated by the relief which purgatives afforded to the patient.

Mr. Gregory Smith had seen the case of a boy, of about 16 years of age, who, for a wager, agreed to drink a pint of gin; when he had drunk about two-thirds of it, he said he had had enough, sat down, and tried to produce vomiting, by thrusting his finger down his throat, but did not succeed in his effort; he shivered, and became insensible; a surgeon was sent for, who requested the assistance of Mr. Smith. On his arrival, he found the lad quite insensible; the pupils dilated; no pulse to be detected, and the extremities cold; the stomach-pump was used, and a large quantity of spirit removed. The patient remained insensible for twelve or thirteen hours, and then slowly recovered. He never, however, from that period was again a strong and healthy lad; on the contrary, his health declined, and he became a burthen to his family.

* See Lancet, page 231, last vol.

Disease of the Eye from Small-pox.—On Variolous Ophthalmia.

A communication on this subject by Mr. Marson was read, but as we inserted the paper entire at page 234 of the last volume of The Lancet, it will be only necessary to observe here, that Mr. Marson denies that the small-pox pustule forms on the conjunctiva; that the inflammation which destroys the cornea in variola is identical with that observed in other of the exanthemata; and that the eye is no more frequently the seat of this inflammation than any other part of the body.

Mr. Hale Thomson said, that the facts brought forward by Mr. Marson were opposed to the general opinion on the subject. He had been in the habit of seeing a great number of patients affected with nebula, who, it was supposed, had originally a small-pox pustule upon the conjunctiva. He recollected, fifteen years ago, when he used to follow Dr. Gregory through the wards of the Small-pox Hospital, he was directed to bleed many patients who were suffering from ophthalmia, supposed to have its origin in the formation of a pustule upon the eye. His impression respecting this mode of treatment was, that in young plethoric patients, cupping was of service. In the generality of cases, however, that state of asthenia existed in which all depletion was contra-indicated, and he had then found the greatest benefit from tonics and the use of fomentations of warm water, followed by the injection of a lotion, consisting of twelve grains of nitrate of silver to an ounce of water.

Dr. A. T. Thomson had found that in the affection in question, neither local nor general

bleeding was admissible; the disease occurred generally in persons whose vital powers were very much reduced. He had found the greatest benefit from purgatives, followed by decoction of bark, with a small quantity of dilute sulphuric acid, and occasional doses of opium. As a local application, he had used a lotion of three grains of nitrate of silver, with a drop of nitric acid, and an ounce of water, injected into the eye, with much benefit. When the disease was treated in its early stages by these means, ulceration would be prevented, and the eye saved.

Mr. MARSON had generally found one application of leeches of service. With respect to the diagnosis of the case, he might mention that, occasionally, when ulceration of the cornea had taken place, the membrane which secretes the aqueous tumour, protruded, and might be mistaken for a pustule. This error was not likely to occur to an observant surgeon, but he knew such mistakes had been made.

Mr. TOYNBEE said, that the accuracy of the results to which Mr. Marson had arrived, by the careful examination of numerous cases of variola, was confirmed by the recent investigations of continental physiologists upon the structure of the conjunctiva.

Mr. G. SMITH did not believe that the facts stated in Mr. Marson's paper were new. He thought that most writers on the subject stated that the purulent ophthalmia which supervened upon small-pox, measles, and other exanthemata, was not different from that which arose when these diseases had not been present. Was there any symptom by which it might be inferred that inflammation was likely to terminate in the destruction of the cornea? The eyelids were difficult to open from the great swelling present.

Mr. MARSON said, that the disease seldom occurred during the time the eyelids were swollen, but generally during the secondary fever, when the swelling had subsided. He knew of no symptom indicative of the occurrence of the mischief, which was frequently discovered accidentally, and when half the cornea was gone, and this destruction was unaccompanied by any appearance of matter from the conjunctiva. Regarding the statements of writers on the subject of the small-pox pustule attacking the eye, he would mention that it was admitted by Lawrence, Middlemore, Morgan, Jacob, Cooper and others, that the small-pox pustule did form on the eye, and that the organ was lost from this cause. The error arose from these practitioners not having seen the disease in its early stage. As to the facts which he (Mr. M.) had brought forward being new, he had met with no writer on the subject who had advanced the same opinion; but on looking over some old volumes of THE LANCET the other day, he had found it stated, that two continental physicians believed that small-pox pustules did not form on the conjunctiva, but no reason was given for this belief.

After a few words from other members on the propriety of naming the disease under consideration "variolous ophthalmia," the Society adjourned until this evening, when Mr. Joseph Toynbee will read a paper on "Diseases of the Ear and their Treatment."

MEDICAL SOCIETY OF LONDON.

Monday, October 21, 1839.

DR. LEONARD STEWART, President.

ERGOT OF RYE—ITS EFFECTS ON THE FŒTUS.

Mr. PROCTER, in reference to the effect of ergot of rye on fœtal life, said, that a friend of his, in extensive midwifery practice in the country, had, from considerable observation, come to the conclusion that it did effect the life of the fœtus. He, Mr. Proctor, thought this conclusion to be well founded. When labour was terminated by this agent, the action was unnatural, the pain was continuous instead of intermittent, and consequently violent. He had been alarmed, in several cases, at the effect of the ergot of rye on the mother. He had seen it in one instance produce delirium and vertigo, and in another umbilical hernia. His friend in the country, to whom he had alluded, instead of taking ergot of rye in his pocket when he went to a labour, had now substituted tea and sugar, and this was acting upon the safe side.

Dr. BENNETT inquired in what way it was supposed the ergot acted injuriously to the infant; was it by producing apoplexy in the mother, or by acting directly on fœtal life?

Mr. STATHAM had administered the ergot of rye in many cases, and he had come to the conclusion that it was injurious to fœtal life. He had at first thought its use admissible in all cases in which the os uteri was dilated to any extent. He now never gave it except when the child's head was in the pelvis, and then he had no doubt of its being advantageous, if the os uteri were flaccid. He considered that the ergot acted injuriously, by producing pressure upon the umbilical chord, and stopping the circulation.

Dr. L. STEWART said, in a discussion which took place in the Society a year or two back, he believed it was a generally entertained opinion that the ergot acted specifically, and had no general physiological action.

Dr. THEOPH. THOMSON said, that the observations of American practitioners upon a large scale, confirmed the opinion that ergot was destructive of fœtal life. The violent and long continued contraction of the uterus, when influenced by ergot, might so effect

the circulation of the mother as to endanger the fœtus. It might be argued that the blood passed very gradually through the placenta, and that therefore this could not be the mode in which the injury was inflicted; but there was still a large quantity of the circulating fluid passing through the placenta in a given time, and it might possibly act in this way.

Mr. PROCTER did not consider that the ergot acted upon the infant as a poison through the agency of the mother. He had seen a case of twins, in which the ergot was given; one child was living, the other dead. Had the ergot acted as a poison through the mother, it would have had the same effect on both children.

Dr. STEWART inquired if any member had employed the medicine under consideration in any other cases than labours.

Mr. STATHAM had used it in a case of chronic bronchitis without effect. It was of great benefit in menorrhagia and abortion, and much superior to acid medicines.

SUDDEN DEATH.

Dr. THEOPHILUS THOMSON gave the short detail of a case which, though in some respects incomplete, he thought would be of interest. He received a hasty summons, about six in the evening, to attend a lady, on whose case it was considered necessary to have a second opinion. On arriving at the house, he found a pale delicate-looking woman, sitting up in bed, with the blood flowing from her arm, and the surgeon in attendance undecided whether or no he should stop it. About ten ounces had been taken away. The patient was breathing with some difficulty, but the breath was not short. The difficulty was not always the same, but variable in degree, and was relieved by the bleeding. The face was pallid, the pulse rapid and weak, but had risen during the flow of blood. There was no tendency to faintness, but there was a dew upon the upper lip. Dr. Thomson advised the flow of blood to be stopped. On examining the chest, no sign of inflammation was present; but a blister had been placed upon the part, under the impression that the patient suffered under pneumonia. The heart's sounds were clear but not strong, and the heart beat over a rather larger space than natural. The abdomen was distended, and elicited a clear sound. He considered the difficulty of breathing to be the result of the tympanitic state of the abdomen, and this opinion was confirmed by her having been occasionally slightly hysterical, and having taken senna-mixture the day before. He ordered her doses of diffusible stimuli; and left her with the understanding, that if she were worse, he was to hear about her again at ten at night. He heard nothing that evening, but received a note on the following morning from the surgeon in attendance, to say, that on visiting her at ten the previous night, she appeared better, but that he had heard, with surprise, that she had died suddenly between one and two that morning. It appeared that she had been relieved by the first dose of the medicine given. Her breathing became worse, and a second dose of the medicine was administered without benefit. Her friends kept her upright in bed; she fell back all at once, and expired instantly. The previous history of the case was incomplete. She was a pale delicate woman, about 30, not chlorotic, and had been well until within the last three months. The menses had been suppressed during that time. Œdema of the ankles had been present during the last month or two, and had gradually increased. She had a slight cough, but no difficulty of breathing, till within a few days of her death. Emenagogues and active aperients had been administered, but no other medicines. Now it would be interesting, as no post-mortem was allowed, to inquire of what this patient died. The case was interesting in some respects. It showed, first, that venesection might afford temporary relief, but be afterwards injurious. This case also was an exception to that class of cases in which the tolerance of blood-letting bore no relation to the necessity for it. What were the circumstances which produced an inaptitude to those impressions which induce syncope? There was no fainting in this case. His own opinion respecting the cause of death was the following: He thought that there was slight dilatation of the heart, so that it had scarcely power to transmit the blood through it; the breathing was rendered difficult, partly from this cause, and partly from the tympanitis. There was probably also an effusion to a small extent in the pericardum before death. The abstraction of the blood relieved the heart for a short time, but more blood was taken than necessary. The patient sunk from a deficient quantity of blood in the system, tympanitic state of the abdomen, and a diseased heart, together.

Some discussion followed on the difficulty of diagnosis between those pains which are merely nervous, and those which have an inflammatory origin, in which Dr. Bennett and Mr. Dendy took part.

At the next meeting a paper will be read on the use of the artemesia absinthium in epilepsy.

THE LANCET.

London, Saturday, October 26, 1839.

It is not often that the proceedings of the College of Physicians deserve to be reported. The Lectures, Orations, *Converzasioni*, have no public interest, and rarely, if ever, encumber our pages; but the policy of the College is a different matter, and should be well understood by Reformers; their principles of government, and their acts as a corporate body, cannot be too vividly impressed upon the mind. We have held it to be a duty to lift the veil, and to disclose the secret councils of the *comitia majora*, upon extraordinary occasions, such, for instance, as the solemn admission of the renegade Licentiates who accepted the Fellowship after having twice petitioned Parliament for the abolition of that invidious distinction. The progress of Medical Reform, and the activity of the liberal Associations, have aroused the heads of the College from a long lethargy; and the events at Court, which we noticed last week, appeared to offer a favourable juncture to the President for a College movement, which might rally the anti-reform forces, and inflict a serious blow upon the enemies of his house and order. It would be idle to report the specious speeches of the intriguing Fellows; so this notice must be considered less as a literal account of the proceedings than as a succinct view of the subtle workings of the system, and a development of the hidden springs by which the Fellows are moved in their public acts.

Suppose a considerable number of the Fellows assembled. Dr. Gordon, Dr. Tweedie, and the renegade Licentiates, seat themselves at the feet of the President, who addresses himself at once to the business of the night. The Elects and Fellows in the room, he says, are well aware that the College had rapidly declined within the last ten years. The public was scarcely conscious of its existence; and the profession regarded its authority neither with reverence nor respect. The College exchequer was at a lamentably low ebb. Empirics had practised with impunity in London, during the last century; but in the present day, *horribile dictu*, even physicians who had acquired a name in science, and were known in other countries, were practising within their jurisdiction, in defiance of the statutes, without the licence of the College; and so strong was the tide of popular prejudice in their favour, that the censors dared not assert their chartered privileges. At this juncture, it would be admitted that the smiles of the Court were more important and indispensible than at any previous period. They required all the influence and patronage which a proximity to Majesty could shed over their officers. Unfortunately, he —their President — was excluded, to this day, from the Palace; a dark cloud intercepted the royal favour; winter was in their souls, and stalked through their desolate halls. The Court was as much misled as the public. It no longer encouraged the venerable body which acknowledged him for its head; and, instead of appointing to the post of Physician in Ordinary their President, —the author of orations replete with classical allusions, and, he must say, not unworthy of the best days of ancient eloquence, Sir James Clark had been placed over him—a physician who had written laborious books upon climate and consumption, without evincing any refinement of taste, any insight into the divine mysteries of Christianity, or, in fact, anything but a knowledge of the method of preventing and curing diseases. When the fatal step was taken, they had, in conformity with their policy, immediately endeavoured to secure Sir James Clark, and to ruin him in the eyes of reformers by tempting him to accept the Fellowship, which he had the ingratitude, or the audacity, to spurn. (Loud murmurs from the renegade Licentiates.) Fortune, or, he would say, a higher power, had led the Physician in Ordinary into an error, which, by a happy co-

incidence, had made a great noise; he was down; and this appeared a favourable moment to strike and crush a friend of Reform—an enemy of the College. He put it to every person present—he asked for a deliberate expression of opinion—was a Fellow of the College ever mistaken? Did he ever fall into a professional error which proved in the slightest degree injurious to a patient? With his hand upon his breast, he would answer for himself, No! Was there a Fellow in his presence who would not answer, No? ("No, no, no," from several voices. A sepulchral voice called out "Lockley," and other names were obscurely heard.) Under these circumstances, what was the policy of the College of Physicians? They must exasperate the public in every possible manner. He would enter into a few details. They must not despise details. Let the Fellows shake their heads in every sick room that they entered; or, if they had not acquired the art of doing that with effect, let them denounce the unhumbled Licentiate in unmeasured terms—day by day. Nor would that be enough. The newspapers exercised an immense influence upon public opinion; every Fellow should write paragraphs, and procure their insertion in the newspapers. This should be dexterously managed. He would explain his meaning by an example. Immediately after the publication of Sir James Clark's statement, he had driven in his carriage to the top of Catherine-street, stepped out, and glided down the street to the office of an Argus-eyed Sunday newspaper; next week a subtle, stinging critique of "the Statement" appeared "from a correspondent," as well as a pungent article. He recommended that to their imitation; to every Christian Fellow he would say, in the language of Holy Writ, "Go thou, and do likewise." [A Fellow doubted the President's prudence, and said that Sir Henry had been seen on that occasion; his excessive precaution had created suspicion. The Fellow was called to order; and the President proceeded.] The Fellows should write popular pamphlets upon the subject. That brought him to a proposition which he had to submit to the *Comitia*. A pamphlet had appeared, by John Fisher Murray, M.D., in which the Court Physician and the Court were as coarsely handled as they could desire. The writer had no knowledge of medicine, and little talent; but he had stigmatised the Court in set terms, and the College had always rewarded its champions in proportion to their zeal, and without any regard to their merits. He would instance one of the Fellows at his feet; for whom he thought John Fisher Murray would prove a highly suitable companion. They would be two for a pair—*Arcades ambo*. He would not read the passage in which Dr. Murray spoke of "us of the profession," or "found "himself compelled to *illuminate* the Court "Doctor," or referred to his experience as a "bedside clerk;" but he considered the expressions, "*blubberings and tears*," "a tri-"umvirate of *viperly* women," and other scorpion phrases, entitled the pamphleteer to the Fellowship. What would they then say to the remarkable phrase, "the authors of this national degradation "are defended and caressed *in high places with impudent obstinacy*!" Could any language be more appropriate, just, or expressive of the sentiments of the College? "Impudent obstinacy!" He would propose that John Fisher Murray be forthwith enrolled among the Fellows of the Royal College of Physicians of London.

Dr. CHAMBERS strongly approved of the phrases which the President had quoted. There was a sort of vulgar sublimity in the abuse. He recommended "impudent obsti-"nacy" to the careful study of their writers; but he doubted the prudence of electing the writer to the Fellowship, upon the specific grounds that he had expressed the feelings of the College, and abused the Sovereign. If they would allow him to use a northern word, he doubted whether it would be altogether *canny*.

Sir JAMES MACGRIGOR and Sir WILLIAM

——agreed in this. They might vote the disloyal Murray a sum of money.

Dr. RODERICK MACLEOD felt himself placed in a very delicate position. They were about to vote a sum of money. He would beg, before they did that, to ask the Fellows present whether they had read the last number of the *Gazette*? He paused for a reply. (Several Fellows replied in the negative. They had paid up their shares, and took it in; but it was too much to expect them to suck the milk of west-end apothecaries. Dr. Clendinning had cut the leaves of his own lectures.) With the permission of the President he would read the last leading article; he had it in his hat.

While the article was being read, several Fellows nodded and were only aroused at the end by

Dr. SEYMOUR, who said, that one remark struck him as original. He would request Dr. Macleod to read it a second time. It was this. "Females will suffer much to be "silently done, that they will not submit to "if it be talked about, and put prominently "before them—in words." With the single exception of that sentence, which he (Dr. Seymour) had never seen before in print, he submitted that the article was spiritless and tame after the pamphlet; it had a lurking air of candour, which was calculated to promote the objects of the Fellows, but he could only propose the vote of a bronze medal to the author. In the actual state of the College exchequer, he approved of the proposition of the President, as did a majority of the Fellows. So John Fisher Murray was elected a Fellow, and the bronze medal was voted; Roderick significantly remarking, that the vote of a few of the QUEEN'S silver, or, if that would not do, copper medals, would have been infinitely more acceptable. He was requested to proceed at once to the residence of John Fisher Murray, M.D., and to bring the happy man to the arms of the President, who was anxious to embrace him in the name of the College.

FANNY HAWKINS spoke to the following effect:—"I have to propose an humble address to the Queen, requesting her Majesty "to dismiss Sir James Clark from the offices "which I and my uncle formerly filled; "my uncle, as I need scarcely remind the "College, appointing me to a post in the "household, from which she dismissed Sir "Gilbert Blane. The error of Sir James Clark "appears to me inexplicable; his crime inexpiable. I regret that we cannot send similar "addresses to all his patients. He could "not decide what was the matter with "Lady Flora, or what the treatment should "be, without an examination. That is admitted. But there were a thousand unexceptionable ways of accomplishing his "purpose. He might have gained the confi-
"dence of the maid; he might ha[illegible]
"his patient in bed some [illegible]
"he might have affe[illegible]
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"corruptible court[illegible]
"more deceptive [illegible]
"speak from pr[illegible]
"very delicate [illegible]
"tached to the [illegible]
"not more circun[illegible]
'Che l'fior virg[illegible]
'Come se lo port[illegible]
"Yet it would have [illegible]
"some, and have led to e[illegible]
"and terror, if my uncle had lis[illegible]
"tales of rival ladies in waiting, and had "carried a cartel of defiance to every pale-"faced sister, 'withering on the virgin "thorn,' simply because she was suspected, "and had a waist less slender in summer "than in spring. The ladies of that day "never would have submitted to the inquisition—I should not—unless I had known "that Sir Charles Mansfield Clarke would "certify, under his hand—the hand of an accoucheur—that ladies '*never had been*,' as "ladies never ought to be that have no "lords to love. A certificate of that kind "might have tempted one; it would have "been novel and piquant."

The motion was seconded, and several Fellows stated at great length how they would

have acted under similar circumstances, and how easy it was to be always in the right.

Dr. ELLIOTSON considered the remarks of Fanny to be frivolous, and that the President would have acted more wisely had he kept his relative at home. When an error had been committed, and the results were flagrant, it was easy to say how it might have been avoided. Thousands were ready to tell the General how a lost battle *might have been* won. He did not get upon his legs to speak to that question. The College would do him the honour to recollect, that he had devoted his best years to the study of Medical Science. He had spent many hours every day in the hospitals; he had delivered clinical lectures. He had introduced several new remedies into practice; their utility was acknowledged. When the stethoscope was scarcely known — or only known to be laughed at—he had lectured upon heart diseases, and upon auscultation, within the walls of the College. He had exerted himself to make the College keep pace with the improvements of the age. (Hear, hear). He had wished to submit to the Fellows, a new means of diagnosis. His discovery was of universal application; it would have been invaluable to Sir James Clark, and would have furnished him with all the information he required. He was led to the discovery in the course of his researches into the nature of *Mesmerism* (Oh! oh!) The girl Okey had a *Negro*. That *Negro* was infallible in his prognosis and in his diagnosis; he had had innumerable proofs of the fact; and he had never known the Negro fail in detecting pregnancy—or even virginity. (Loud cries of "Oh; nickel! nickel! nickel!") That explained the ancient fable of the Lion and the Virgin. He would not be put down by clamour. Truth had always been persecuted within those walls. (Cries of "Order, order.")

The PRESIDENT was bound to call Dr. Elliotson to order. He must, however, say, he saw less to object to in the new mode of diagnosis than in many other novelties which Dr. Elliotson had encouraged. The stethoscope, for example, was an annoying innovation. He understood that Dr. Elliotson practised several months upon that instrument before he could employ it with confidence. That was the case with discoveries. They gave him, and the heads of the profession generally, immense trouble, without adding a doit to their fees. It was true, that the Negro would give little or no trouble. It was an easy way. You had only to put your questions to Okey, and to listen to the answers, if he had understood Dr. Elliotson. He would now ask Dr. Elliotson as the mesmeric Negro was but a spirit—a shadow—whether a *real Negro* would not be a more efficient and convenient agent? Instead of carrying Okey in your carriage, you could then mount the Negro upon the box, and call him in to cases of difficulty. (Dr. E. said that the part of Africa in which Okey's Negroes lived, had not yet been discovered). He (the President) would say no more upon the subject. He had a project which would destroy medical discoverers. It was not sufficient to exclude them from office in the College, nor to raise blockheads to the Fellowship, who could neither be rivals nor give trouble. They must employ more active measures. His strongest objection to medical reform was the manifest tendency it had to promote discoveries of every kind. The Senior Fellows must then be incessantly learning; they could never lay their heads down in peace; and it was too much to expect men with hoary locks to make love to medical philosophy; or to address her as the venerable Anacreon addressed his mistress:—

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'Che l'fior virginal cosi avea salvo,
'Come se lo portò del materno alvo;'

"Yet it would have been insufferably irksome, and have led to endless uneasiness "and terror, if my uncle had listened to the "tales of rival ladies in waiting, and had "carried a cartel of defiance to every pale-faced sister, 'withering on the virgin "thorn,' simply because she was suspected, "and had a waist less slender in summer "than in spring. The ladies of that day "never would have submitted to the inquisition—I should not—unless I had known "that Sir Charles Mansfield Clarke would "certify, under his hand—the hand of an accoucheur—that ladies '*never had been*,' as "ladies never ought to be that have no "lords to love. A certificate of that kind "might have tempted one; it would have "been novel and piquant."

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but he deemed it right to employ more caution than the President, in visiting a scurvy acquaintance. He had left *his* carriage at the coachmaker's, and had walked. He had not been able to find any John Fisher Murray, M.D., in Park-street; and from all he had learned, was led to believe that the aforesaid was absolutely "a man of straw."

Dr. SEYMOUR, glancing sarcastically at Roderick, and the renegade Licentiates, observed that this was not the first "man of straw" that had been made a Fellow.

The election was cancelled, and the address to the QUEEN was carried.

The PRESIDENT said, it only remained to name a successor to Sir J. CLARK, and he apprehended that they would have no difficulty in selecting a much more competent individual. Every Fellow would write down the name of the person, who, he conscientiously believed, was best qualified to fill the responsible office. This was done without any hesitation. The names were collected in the ballot box, and Dr. WATSON, who was appointed scrutator, announced the result. It was the most singular, he said, that he had ever witnessed. Every Fellow present had the same number of votes——one. The question remained undecided. The President, after a pause, bit his nether lip with rage, exclaiming, "'tis too bad! this ingratitude will kill me. It is quite clear that every Fellow wrote his own name upon the slip instead of mine. After years of devoted service the College abandons me. I will turn Radical!" Recovering by slow degrees, he dissolved the *comitia*. They had done nothing. They could not address her Majesty, nor request her to dismiss Sir James Clark, as they had been unable to agree in nominating an abler successor.

So it has already come to this, that the Examiners of our liberal Metropolitan University, who conduct their scrutinies *in secret*, dare not furnish a candidate whom they decline to put in the "First Class," with copies of his answers to their questions, nor will permit him to take copies for himself! Our predictions when the charter was first granted, severe as they were, did not forbode such a degree of degradation as the University of London has already reached. There must be a new charter for this institution, and a new system adopted, ere it can confer an honour on any candidate for its diploma, or supply one useful result to the public.

We give insertion to the following letters, because the conduct of the examiners in the case of Mr. MACKENZIE, who has proved that he was a highly qualified and fearless candidate for the degree of M.B., cannot be too frequently brought before the scientific portion of the community. It is useless to declaim against the ungenerous and disgusting treatment which Mr. MACKENZIE experienced, but it is not useless to direct attention to every fact which is calculated to exhibit the conduct of the examiners towards him. At our very first examination of the charter of the University, we foretold what would be the character of the institution. The discussions which led to its concession, were marked throughout by the purest aspirations of liberality. Accusations of narrow-mindedness, and bigotry of the worst order, were directed against the Universities of Oxford and Cambridge; but the New University of *London* was to arise before the enlightened people of the empire, as a temple which should afford a shelter to every candidate for scientific and literary fame, and shame into improved practices the corrupt and benighted Universities of Isis and the Cam. And what do we now find? Why, that the latter universities are paragons of liberality compared with their infant rival of the metropolis. In the ancient institutions there is nothing of *pretence*, no mockery of a generous system; they offer no insults to candidates for honours, by urging them to hope for advantages which it is never intended that they shall receive. In the ancient institutions there is nothing sham. In the modern, all is deception.

The letters will speak for themselves. Though the justness of the demand made by

Mr. MACKENZIE has not been satisfied, yet the triumph which he has obtained over his unworthy persecutors is signal, and will be lasting. Their own self-condemnation renders his victory effectual and complete.

"To the Senate of the University of London.

"My Lords and Gentlemen:—As I have reason to fear, from the letter of your Registrar, dated Aug. 1st, that the grounds of the memorial which I addressed to you on the 31st of July last, have been entirely misunderstood, and as no alteration in the division of classes, to which I therein referred, appears to be contemplated, notwithstanding the unequivocal declarations of some of the examiners, that many of the candidates were, by the decision of the board, subjected to great injustice, I take the liberty of respectfully requesting your permission, either to allow me to be supplied with, or to make myself, copies of my answers at the second examination for the degree of M.B., with a view to placing them before the medical public, whose opinion as to the position that I ought to occupy among the candidates, may be to me of vital importance. I have the honour to be, my lords and gentlemen, with the greatest respect, your obedient servant,

"F. W. MACKENZIE."

"London, Aug. 23, 1839."

"To F. W. Mackenzie, Esq.

"Sir:—The Registrar of the University of London has directed me to state to you, that the Senate having adjourned to the 16th of October, and the chancellor and vice-chancellor being both absent from town, he is unable to do more, at present, than to acknowledge, on the part of the Senate, the receipt of your communication of the 23rd inst. I have the honour to be, Sir, your very obedient servant, "H. MOORE.

"University of London,
Aug. 27, 1839."

"To F. W. Mackenzie, Esq., University College.

"Sir:—In answer to your letter of the 23rd of August, addressed to the Senate of the University of London, in which you request the permission of the Senate either to allow you to be supplied with, or to make yourself, copies of your answers at the second examination for the degree of M.B., I am desired to inform you, that the Senate declines acceding to your request. I remain, Sir, your very obedient servant,

"R. W. ROTHMAN, (Registrar.)

"University of London,
Oct. 17, 1839."

*** The ground taken by Mr. MACKENZIE in his memorial to the Senate, published in THE LANCET of July, 1839, was thus stated in the introductory paragraph:—He said that, "he had reason to believe that, in drawing the line between the first and second class of candidates, the examiners had been influenced *by other considerations than those relating to the* MERITS *of the candidates*; he, therefore, prayed the Senate to inquire into the matter, and, if his belief was well-founded, to revise the decision of the board." The answer of Mr. ROTHMAN on that occasion, was thus worded:—"Sir, you are mistaken in the grounds of your memorial, as you were not placed in the second class *on account of the omission of the Latin subject of examination*,"—an *answer* to the application of Mr. MACKENZIE, which would be rather extraordinary, were it not of a piece with all the other shifts, shuffles, and evasions of the board, throughout this rank transaction.

TO THE READERS OF THE LANCET.

35, Bedford-square, Oct. 19, 1839.

GENTLEMEN:—A strong sense of duty, and the high respect which I entertain for the opinions of the members of the medical profession, irresistibly impel me to address to you a short series of letters concerning the very gross misrepresentations which have lately appeared in the columns of many of the newspapers, relative to the manner in which it is alleged that I have exercised some of the functions which belong to the office of Coroner. Several writers, from motives that are best known to themselves, have made the press the medium of conveying to the public many statements which, if they remain unexposed and uncontradicted, might operate injuriously with reference to the utility of the Court in which I have the honour to preside. I, also, therefore, must resort to the press, as the instrument of my defence, and endeavour to exert its great and mighty influence towards maintaining the power and usefulness of an institution which can claim the evidence of many cen-

turies in proof of the numerous benefits that it has conferred on the public. In replying to the false statements which have been circulated, I tell my opponents, that I desire nothing so much as scrutiny,—searching scrutiny. The mode of attack which they have adopted has been of a very dastardly nature. *Specific accusations* there have been none; but criminations, and appeals to bad passions and vulgar prejudices, have been sufficiently numerous. It shall be my object, in the letters which I am about to address to you, to remove those erroneous impressions in the public mind, which are the results of the false and malicious statements that have been propagated, in anonymous letters, scandalous paragraphs, and unauthenticated "reports."

The duties of the Coroner's office have been brought prominently before the public within the last fortnight, in consequence of the discussion which took place at the bench of magistrates, on the 11th instant, in the Sessions House, Clerkenwell. The unjust allegations which had been made against me elsewhere, were repeated on that occasion by some of the justices, who did not shrink from adopting the charges, and urging them in argument before their brother magistrates.

The subject was introduced at the Sessions by the Rev. THEODORE WILLIAMS, vicar of Hendon, who made it a matter of grave and serious complaint, that an inquest had been held in his parish on the body of THOMAS AUSTIN, a pauper who had fallen into a copper of boiling water in the Union workhouse. That magistrate, clergyman, and chairman of the board of guardians, having denounced the inquest on AUSTIN as an "unnecessary" and illegal proceeding, he was followed in the discussion by his brother justice, Mr. LAURIE. I shall, however, place all the particulars of the "Hendon case" before the public on another occasion, and then not only prove, to the satisfaction, I trust, of every intelligent person the gross and lamentable ignorance which the vicar of Hendon exhibited at the inquest in question, relative to the duties which of right belong to the office of Coroner, but I shall also expose the palpable and disreputable inconsistency and negligence of his conduct on that occasion. In the present letter I shall content myself with replying to the statements which were made by Mr. LAURIE, echoed and re-echoed as they were by him and others; and when it is remarked that his observations were made in immediate connexion with the complaint which had just been urged by the Rev. THEODORE WILLIAMS, the whole force of his allegations respecting the imputed increase of expenditure from holding inquests during the *last* year, seems to have been levelled against me, and made condemnatory of *my* official conduct. Allegations more untenable, or more utterly groundless, were never uttered, and few have ever been advanced by any party which could reflect more discredit on the accusers. Instead of becoming the trumpet of one or two low-minded paragraph-mongers who traffick in falsehood, Mr. LAURIE, as became a person of rank and station, should have investigated, with patience and care, the whole of the subject, before he came forward in the invidious character of an accuser against a public officer who had faithfully discharged his duty. Mr. LAURIE will discover, when it is too late, that he acted a part in the Middlesex Sessions House, from which an honourable man might instinctively have shrunk, and which no gentleman of integrity, whose mind had been fortified by an ordinary share of observation and knowledge, should have thought of occupying, even for a single moment. A plain statement of facts, and a reference to truth-telling figures, will expose the motives of the pretended economists and real hypocrites who have joined in the recent senseless clamour on the two exciting topics of "increase of expense," and "unnecessary Inquests."

It suited the purpose of Mr. LAURIE and his prompters to contrast the payments made out of the county fund in the years 1835-6,

with the payments which were made from the same source in 1837-8. This contrast, as I have observed, followed immediately upon an accusation against me to the effect that I had unnecessarily held an Inquest upon a man, aged 79 years, who had been fatally scalded in one of the coppers of the Hendon Workhouse. Was the conduct of Mr. LAURIE in this proceeding that of a candid opponent?

A spirit of fairness would have induced Mr. LAURIE and his supporters to institute a comparison between the average weekly Inquests which *I have taken* since my appointment in February last, and the average number which were taken *by Mr. Stirling in* 1838; or between the Inquests held *by me*, and the average weekly number taken by the Coroner for the eastern division in the *past and present year*. But such a course of demonstration did not suit the purpose of my opponents. *Why*, will be seen presently. It was more convenient for Mr. LAURIE and his coadjutors to enlarge upon "the *increase*" within the *last year and a half*, as compared with the Inquests which were held in the years 1835-6. I am obliged to appeal from the injustice of such proceedings, to the good sense and honest convictions of the public. I am compelled to turn from the unwarrantable inference which must follow this scandalous mode of attack, to *facts*, obvious, indisputable facts, sustained, as they are, upon their only true basis, exact numbers and precise dates. "Look here!" exclaims a candid specimen of a justice of the peace; "The sum "paid for Coroners' Inquests out of the "*county rate*, for the year 1836, was "£1,177. 13s. 6d. For the year 1838 it was "£4,134. 16s. 3d. Is not this increase most "alarming?" And thus he tries by inference to sustain the complaint of the vicar of Hendon, that "the increase" in the sum arises out of *an augmented number of "unnecessary Inquests."* Can any excuse be offered for a commentator who, seeing that a reference to *facts* would defeat his object, thus dispenses with them while they are within his reach, and at his own command and disposal? Is he so ignorant, can he be so ignorant, as not to know that £2,384. 11s. 3d. out of the £4,134. 16s. 3d. which he quotes has been, since 1837, a *new charge* upon the county-rate, arising *out of the passing of the Poor-Law Amendment Act, and the consequent Act of* 1 VICTORIA, c. 68? A few words will explain this matter very satisfactorily. No sooner was the new Poor Law in operation, than the Poor-Law Commissioners refused to allow any payments *for Inquests* to be made out of *the poor-rates of the respective parishes*. Not one of those payments was permitted to remain in the accounts. Under the new law all those allowances and payments were at once discontinued. What was the consequence? As no provision existed for meeting the necessary expenses of the court, the Coroner often experienced considerable difficulty in finding rooms wherein to pursue his inquiries, and in many places dead bodies were left exposed in out-houses, to the annoyance of a whole neighbourhood, and the disgrace of the country in which such scenes were witnessed. A general outcry having been raised against these revolting occurrences, the attention of government was directed to the subject, and the present solicitor-general, Sir M. ROLFE, introduced into the House of Commons "A Bill to provide for the payment of the "expenses of holding Coroners' Inquests," which Bill, having received the royal assent on the 15th of July, 1837, provided, thenceforth, that the justices of the peace in every county, and the town council in every borough having a Coroner, should "make or "cause to be made a schedule of the several "fees, allowances, and disbursements which, "on the holding of any Inquest on any dead "body within such county or borough, "might be lawfully paid and made by the "Coroner."

In conformity with this enactment, the magistrates of Middlesex, in November, 1837, prepared a schedule of fees and allowances to be paid to witnesses and other persons at Inquests.

In the same Act, 1 Vict., c. 68, it is provided that the Coroner, in consequence of the advances, out of pocket, which he was thus called upon to make, and for the additional trouble and loss of time which that regulation would occasion, should receive 6*s.* 8*d.*, in addition to his previous fee of £1 on each inquisition.

Thus, then, it appears that previously to 1837, whilst the only payments which were made out of the *county-rate* relating to Inquests, were those of *the Coroner's fee of* £1, and his *mileage*, there now is issued from the same source, *all* the expenses of the Inquests which are held throughout the whole county—in fact, all those costs and charges which, before the Poor-Law Amendment Bill was passed, were paid out of the poor-rate, by overseers and vestrymen, in their respective *parishes*.

This plain explanation of the increase of the demand on the *county-rate*, surely must have been known to my opponents. Why were they silent on the subject? The omission of this explanation is not calculated to obtain respect for them or their statements on future occasions.

The foregoing statement is due as well to my honourable and venerable predecessor in the office of Coroner, and to all the other Coroners of the kingdom, as to myself, because the "orders" of the Poor-Law Commissioners had a general operation, and have caused a discontinuance of the payments for inquests in all the parishes of England and Wales, precisely in the same manner as they affect, in this respect, the *county-rate* of Middlesex. I hesitate not to say, that this explanation ought to have been furnished by the magistrates themselves. To me it is incomprehensible how a body of gentlemen, of high character and station, could listen to statements which were so well calculated to operate injuriously upon the characters of some public officers of the county, without offering the simple exposition of facts which I have here presented to the consideration of the public.

Having shown the effect of the transfer of the payments from the poor-rate to the county-rate, let me ask, when was the person who has now the honour of addressing you elected to the office of Coroner for Middlesex? Why, on the 25th of February in the *present* year. Yes. I have occupied the office during less than eight months, and yet it has been deemed proper in the Middlesex Court of Sessions, and, out of it, by some anonymous slander-mongers, to associate *my* name with the alleged increase of inquests in the year 1838! A more unjust attack surely never was attempted. Happily, its absurdity is fatal to its intent, and the effects of the intended blow must fall in some other direction. For myself, I court, I solicit investigation. I have no cause to shrink from the discussion of this subject. I shall, therefore, proceed to place before you the number of inquests which have been held by me, and compare them, first with those which were taken by my predecessor, Mr. Stirling, and, at another time with those inquests which have been held by coroner for the Eastern division of the the county. Previously, however, to making these comparisons, it may be right to explain the course which, on entering upon the duties of my office, I thought it right at once to pursue.

Briefly, then, I may state that Mr. Stirling, my predecessor, was 91 years of age when he died. He performed the duties of his office until within six days of his decease, in January last. His private residence was at Battersea; his office in Chancery-lane. A gentleman, named Bell, acted as clerk to Mr. Stirling during the last fifteen years of the life of the late Coroner, and continued in that capacity up to the day of Mr. Stirling's dissolution. During the larger portion of that time, Mr. Bell attended the office of Mr. Stirling, where he received from the constables and beadles all the applications for inquest warrants. Excepting in very peculiar cases, Mr. Bell was enabled, from his intimate acquaintance with the business of the court, to grant or refuse warrants, with-

out consulting Mr. STIRLING. He also made out the accounts of the Coroner, and transmitted them, with the necessary vouchers, to the magistrates at the Sessions. Under these arrangements, no complaint was heard at the great house in Clerkenwell; not a murmur of dissent escaped from the lips of the magistrates, either after the death of Mr. STIRLING, or at the discussions which took place at the Sessions' House in February last, immediately preceding the last election for Coroner. On that occasion, it is true, a reference was made to an increase in the number of inquests which the accounts had exhibited in 1838; but this circumstance was not mentioned in *a tone of condemnation*. It did not then elicit one word of anger or reproach, either with reference to the living or the dead.

On ascertaining, immediately before I was sworn into office, that Mr. BELL had occupied this prominent and active position with my aged predecessor, I engaged him as my clerk. On resuming his duties in Chancery-lane, Mr. BELL was, on the very first day that I exercised the functions of Coroner, expressly instructed to act for me as he had acted for Mr. STIRLING during the fifteen years of his engagement with that gentleman. Accordingly, Mr. BELL attended at the office, and there received the applications from constables, for me, as he had done for Mr. STIRLING; granted or refused to supply the warrants, for me, as he had done for Mr. STIRLING; and prepared the accounts, and sent them to the magistrates, with the necessary vouchers, for me, precisely as he had framed them and sent them to the Sessions for Mr. STIRLING. While all these things were being executed *for the late Coroner*, nothing could be more excellent. The duties of the office were *then* efficiently and admirably performed. But now that similar labours are fulfilled for the *medical* Coroner, *every thing is wrong*, and deserves *animadversion*. It is fortunate, under these circumstance, that I can appeal from the prejudices which are entertained by some of the justices of the county of Middlesex, to the sound sense and just feelings of an intelligent and right-thinking community.

Notwithstanding the outcry which has been raised against the "enormous number of unnecessary inquests" that are alleged to have been taken since the medical Coroner has filled the office, *only one inquest, of all that I have held, has been the result of my own appointment*. All the warrants for holding inquests, without another exception, have been granted to the constables by Mr. BELL, in precise and exact conformity with the rules of practice, which he observed during so many years in the office of my venerable predecessor. So thoroughly and completely have I carried out this arrangement, that, from the day of my election down to the present moment, my foot has not once crossed the threshold of the office in Chancery-lane.

Had it been known in a certain quarter that my arrangements were such as I have described them to be, it is more than probable that some of my opponents at the Sessions would have remained discreetly silent; for, instead of injuring *me*, by gross and scandalous misstatements, distorted facts, and unjust inferences, they have contrived to adopt a course of conduct which should overwhelm them with public censure. No honest politician, however strong may be his opinions, or violent his prejudices, after what I have stated will feel aught but disgust on noticing the behaviour of certain of the magistrates of this county. The agitation of the subject was not commenced by me. I did not solicit criticism, hostility, or approbation, from their voices. Clear it is that I have no desire to become the victim of incessant calumny and misrepresentations. The statements which I have just made relative to the exactitude of my arrangements for granting warrants, with those of Mr. STIRLING, and the position which Mr. BELL has occupied with my predecessor and myself, have been forced upon me. If a perusal of them should excite the blush of shame in the countenances of any of the magistrates, the fault is not mine. I am only acting in self-defence.

After perusing the explanation which I have now furnished to you and to the public, very many persons may be induced to exclaim, "If Mr. BELL has granted warrants for inquests precisely for you as he did for Mr. STIRLING, how has it happened that your inquests have so prodigiously outnumbered those of your predecessor?" What is the answer to this plain and very natural question? What is the brief but conclusive reply to this pointed interrogatory? Here is the answer. The inquests which have been taken by me since I have been elected, down to the hour at which I am now writing, have averaged, throughout, *one a week less than the inquests that were taken by Mr.* STIRLING *in the last year of his life*, 1838. If the publication of this fact does not confound some of the lie-mongers of the day, their effrontery is of so hardened a nature that they must be alike insensible to shame and reproof. Any of the magistrates who may feel inclined to question the accuracy of my statement concerning the number of inquests taken by me and my predecessor, may remove their generous apprehensions, may gratify their unsophisticated curiosity, by referring to the Coroners' accounts for the past and present years. What other evidence could they desire? It may, I know, be unsatisfactory to them; but, assuredly, the facts which I have disclosed in this letter will prove to you and to the public that I have of late been most scandalously misrepresented in a large portion of the journals which are printed in this metropolis.

You must now understand the character of the hostility which I am encountering, and you will perceive that the weapons which are employed against me are slander, misrepresentation, and falsehood. You cannot fail to discover that I have given no such offence as that of taking "unnecessary inquests," or exceeding the number which were presided over by the late aged occupant of my office. No; the spirit of opposition has been engendered by, and has emanated from, another source. The crime of which I have been guilty,—and it is one of no trifling magnitude in the opinion of some persons,—is, that I have dared to put into practice those powers which legitimately belong to the office which I have the honour to hold. In a word, the *medical coroner* is not better liked by *some* of the magistrates in the month of October, than was the *medical candidate* by the self-same magistrates in the month of February. The veil which was thrown over the late proceedings at the Sessions, is of too flimsy a nature to conceal the palpably naked and odious motives of some of the actors on that occasion. A momentary reference to the proceedings of February last, in the same quarter, by some of the same persons, must lay bare the mean and unworthy objects of the calumniators.

When I was a candidate for the office of Coroner, in the month just named, the "increase of inquests" in 1838 was made the subject of two or three animated discussions by the justices. The numbers taken by the two Coroners for the Western and Eastern divisions of the county had been ascertained from the books, and were proclaimed to the public in oratorical addresses from the Bench. The *utility* of the office of Coroner was acknowledged in profound and learned speeches. What on that occasion was proposed to be done? Were the feelings of the present agitators shocked by the alleged "increase" in the number of inquests? Alas! the current of philanthrophy did not then flow in *that* direction. Was it proposed to *abolish* the office of Coroner, by reason of the additional expense to the county in the year 1838? Alas! A feeling of economy for the rate-payers did not then form any portion of the argument. What, then, was the scheme of a majority of the honourable, wise, and unprejudiced justices who assembled at that important crisis in the affairs of this great county? Why it was resolved to appeal by "memorial" to the LORD HIGH CHANCELLOR. For what? To get rid of the office of Coroner? To put down so costly a burthen? Not at all. What, then, was the proposal of the wise justices, in order that there might in future be a *less number of inquests* and a

smaller amount of expense? Why, they humbly but fervently prayed that TWO CORONERS *might be elected, to supply the* ONE *vacancy which then existed in consequence of the death of* Mr. STIRLING! A marvellously sagacious project for diminishing the expenses of the Coroner's office, and decreasing the number of inquests!

When the *recent* proceedings at the Sessions House are thus thrown into juxta-position with the events which occurred there *in February*, the scene which is produced renders my triumph complete, and the exposure of the purposes of my opponents signally effectual. When political or other hypocrites, in the blindness of their ignorance and folly, labour to UNMASK THEMSELVES, they sometimes require a little friendly help. If such characters should derive no advantage from my assistance, the fault shall be their own, not mine.

I have the honour to remain,
Gentlemen,
Your faithful and obedient servant,
THOMAS WAKLEY.

*** Since the above letter was written and put into type, I have been informed, by a magistrate who was present at the sessions, that the reports of the discussion which appeared in the newspapers, exhibited a garbled and exceedingly unfair account of the proceedings. It has also been stated to me, that the Vicar of Hendon was my only enemy on the occasion. Both of these communications are alike gratifying to me, and if in my remarks, I have spoken unjustly of the conduct of any magistrate, I shall not be slow in making the *amende honorable*.

FORGERY OF AN APOTHECARY'S LICENSE. IMPUNITY OF THE CULPRIT.

To the Editor of THE LANCET.

SIR:—Should the following facts be deemed of importance, I pledge myself for their veracity, and am ready to furnish the names of the parties alluded to, to any person who is properly authorised to receiv them; they have recently come under m notice in the county of York. An individua. who had practised elsewhere as a physician, settled in the place from which I write, as a general practitioner, and shortly afterwards became a candidate for the situation of visiting surgeon to our dispensary. His certificate of qualification, from the Apothecaries Hall of London, being required to be shown, the alleged licence was submitted for inspection previous to the day of election; but, some suspicion afterwards arising on the subject, an inquiry was instituted, and it was found that no such person as the candidate had ever received such a document from the court in question. It was afterwards clearly proved that the document was forged, having originally belonged to a deceased cousin, and subsequently, by a dexterous alteration of names, rendered subservient for a second purpose. The "Worshipful Company," upon learning this fact, waxed exceeding wroth, and commenced a prosecution against the individual, to support which a legal gentleman was appointed in the town to collect evidence. After a lengthened correspondence with the secretary of the society, that officer at last informs us, that the society do not intend to prosecute, as it is the opinion of the counsel, whose advice they have taken on the subject, that, "although the person was undoubtedly guilty of uttering a forged certificate, they cannot discover any mode of describing the object to be obtained by it, which will constitute an indictable offence." The Apothecaries Company, however, generously offer the evidence procured, to any gentleman in the town who feels desirous of prosecuting the culprit!

Mr. Editor, the medical session of 1839-40 has just commenced. Hundreds of students are now entering upon their curriculum of study in England, and are looking towards the diploma of the Apothecaries' Company as the *ne plus ultra* of their ambition. Some, doubtless, in that pursuit, are spending their little all; whilst others are sacrificing the hard-earned pittance of a parent's toil, to effect their object. And what is to be attained by it? A piece of paper, forsooth; which one man may obtain from another, and pass off as his own, and, as in the present case, laugh at any attempt to dispute the mode in which it was procured. You, Sir, have ever been considered as the "Student's Friend." By exercising your talents and energies in putting down a system under

which frauds such as this go *unpunished*, you will lay an additional claim to the above title, gain the thanks of the profession at large, and command the esteem of all honourable men. I am, Sir, your's very respectfully,

A JUNIOR PRACTITIONER.

Yorkshire, Oct. 7, 1839.

BRITISH MEDICAL ASSOCIATION.

Exeter Hall, October 23, 1839.

AT a numerous meeting of the newly-elected Council of this Society,

R. DAVIDSON, Esq., was unanimously called to the chair.

The Chairman read over the names of the following Gentlemen, as forming the Council for the ensuing year:—

Bainbridge, J. Esq.	Hilles, M. W. ..
Bayfield, J. S. ..	Hooper, R. L. ..
Beane, J. M. ..	Howell, Jos. ..
Bristowe, S. J. ..	Hull, George M.D.
Brady, Chas. ..	I'on, L. J., Esq.
Coleby, T. T. ..	James, W. P. ..
Crisp, E. ..	Lynch, J. M.D.
Davidson, R. ..	M'Cann, N. Esq.
Eales, Wm. ..	M'Ewan, D., M.D.
Evans, E. ..	Parsons, G. Esq.
Eisdell, N. ..	Pilcher, G. ..
Farr, W. ..	Rendle, — ..
Grainger, R. D. ..	Smith, S. M.D.
Granville, A. M.D.	Simpson, —— Esq.
Grant, Robt. ..	Stevens, R. ..
Green, P. H. ..	Wagstaffe, M. ..
Hall, Marshall ..	Wakley, T. M.P.
Harrison, C. Esq.	Webster, G. M.D.

The CHAIRMAN informed the Meeting that the first business of the evening was the election, by ballot, of a President, Six Vice-Presidents, and Secretary.

The SECRETARY (*pro tem.*) declared the result of the balloting list as follows:—

GEORGE WEBSTER, Esq. M.D. President.

VICE PRESIDENTS:

R. Davidson, Esq.	A. Granville, M.D.
M. Hall, M.D.	J. Howell, Esq.
G. Pilcher, Esq.	R. Grant, M.D.

C. H. R. HARRISON, Esq., Hon. Sec.

The Minutes of the last Meeting now being read,

The CHAIRMAN read over the names of those Gentlemen who were elected Members of the Association at the General Meeting on the 7th October ult.

Dr. P. Johnstone, Shrewsbury.
W. T. Elliott, Esq., Old Brompton.
Wm. Tucker, Esq., Market, Leamington, Wilts.
Jas. Sharp, Esq., Grosvenor-place, West.
Wm. Grimwood King, Esq., 7, Thurlow-place, Hackney-road.
Wm. Tidy, Esq., Providence-place, Mare-street, Hackney.
Wm. Huxtable, Esq., Hackney.
Donald Fraser, Esq., Polygon, Somers' Town.
Dr. Reid Clanny, Sunderland.

The Council then resolved itself into a Reform Committee, and having elected R. Davidson, as Chairman, appointed a Sub-Committee to watch over the interests of Reform, consisting of the following Gentlemen, with power to add to their numbers:—

Dr. M. Hall.	Mr. Wakley, M.P.
Dr. Webster.	Mr. R. Stevens.
Mr. Pilcher.	Mr. Simpson.
Mr. Farr.	Mr. Davidson.
Dr. Granville.	

The Meeting having again formed itself into one of the Council, numerous letters lying on the table on the subject of Medical Reform from various parts of the country, were then referred to the consideration of the Sub-Committee on Medical Reform.

The meeting then adjourned until Tuesday, the 5th November.

ST. BARTHOLOMEW'S HOSPITAL.

COMPOUND FRACTURE OF TIBIA.—EARLY BANDAGING.—REMARKS ON IMMOVEABLE APPARATUS.

RICHARD ROBINSON, age 17, was admitted Oct. 12, with a compound fracture of the lower third of the tibia, produced by a blow from the pole of an omnibus. The wound in the integuments was not large, but there was a good deal of grazing of the skin, and some swelling. On looking into the ward an hour afterwards, we found the leg put up with a splint on each side, and padded with tow; simple dressing being applied over the wound. The next day the boy was feverish, and as he was suffering pain from the tightness of the bandage, caused by increase of the swelling, one of the splints was removed. Nothing particular has occurred since, the case going on very well, and he now has on splints so formed as to allow the application of poultices over the wound.

This case is scarcely worth recording from its intrinsic interest, nor is it so well calculated as some others which have lately occurred in St. Bartholomew's, to show the evil effects of putting up fractures *early* in the ordinary manner. There was one lately where the flesh had risen completely through the interstices of the splints, of course producing much suffering. Indeed, the great uneasiness and pain very generally produced by the swelling which follows early bandaging, and is no doubt frequently increased by it, have led many of our most experienced surgeons simply to place the limb in a suitable and comfortable position, for the first few days, preventing and subduing inflam-

mation either by poultices or leeches and cold lotions. Some have gone so far as to lay aside the use of splints, or any other apparatus whatsoever; and, no doubt, have, in many cases, succeeded well. There is now under the care of one of the surgeons, a man with fractured humerus, which has been simply laid on a pillow, and has united without the slightest deformity. On the other hand, in the accident ward of Guy's, is a foreigner with a fractured thigh, which had been placed on the double inclined plane on pillows, who appears likely to suffer greatly from the practice. There now exists very considerable deformity, and it is very improbable that it is caused solely by the amount of the callus. Any method, then, likely to combine the advantages of both plans of treatment, would be a decided improvement.

The writer has seen several cases leading to the conclusion that the objections against early bandaging do not apply to the plan, long ago practised by Cheselden, and lately revived in this country, of treating fractures by an immoveable apparatus. This is a very simple and efficacious mode of proceeding, well adapted for hospital and country practice; indeed, in most cases where the comfort of the patient is regarded rather than the pocket of the practitioner. It consists simply of calico-rollers, wetted with mucilage of starch, and applied moist after the adaptation of the fractured extremities of the bone. The bandages are then soft, and adapt themselves to every irregularity of the surface; but on drying, which takes place in from 24 to 48 hours, they form a hard case, which effectually prevents motion; so that the patient can move about on crutches, after a few days, without the slightest danger of producing displacement. It is plain how much this must add to the comfort, and conduce to the general health of the sufferer.

Some similar mode was evidently practised by the Egyptians, as mummies have been found with fractures put up in an immoveable apparatus, probably a solution of aromatic resins in spirit, with which the swathing cloths were imbued. And among the Arabians and Persians, to the present day, this mode is practised. Baron Larrey was the first who introduced it into Europe, after he had used it extensively in Egypt. He was led to adopt the plan from the necessity he frequently had for moving soldiers with bad compound fractures, great distances on dromedaries, and other shaking conveyances. It answered perfectly; and his son, who followed up the practice, records the case of a person who was rapidly conveyed a hundred miles without injury, the day after the accident. Larrey's stiffening was a mixture of white of eggs and camphor mixture.

With the exception of some awkward attempts in this country, by means of fracture boxes and plaster of Paris, we do not hear much more of this practice till the last siege of Antwerp, when the chief surgeon of the besieging army found a necessity for adopting such means. He applied pieces of lint, wetted with liquor plumbi, next the limb, over this a starched bandage, then two wetted pasteboard splints, and over this another starched bandage. This somewhat clumsy proceeding, is still the general practice in Brussels, and in many Belgian hospitals. It was also commenced in the hospitals of Dublin, but now a modified plan is carried on there. The lint and pasteboard are omitted, one or two starched rollers alone being used. So far, this mode has answered admirably, and it would appear that the sooner the fracture is put up the better; the danger of swelling, compression, strangulation, &c., being far more imaginary than real. More experience, however, is wanting on this point, though we know the same ill effects do not follow this early bandaging, as come on after splints, &c. are applied early. Perhaps this may be owing to the pressure and support being, in the one case, uniform and regular, and in the other only partial.

In the hospitals of Brussels and Hamburgh, if a fracture be brought in, the swelling of the limb being already great, the starched bandages are immediately applied, just as if no swelling existed. The tumefaction goes down in a few days, when a V shaped piece is cut out of the now hardened case, and a fresh bandage applied over the remainder. Compound fractures are also treated in the same manner. In these cases, if suppuration come on, a little pus exudes, which being excluded from atmospheric air, does not putrify, but forms a scab, a natural and capital dressing, and the wound often heals by the first intention; the case recovering almost as soon as a simple fracture of the same bone. The writer once saw a compound dislocation of the ankle joint put up thus immediately after the accident, and recovery followed, without a single bad symptom.

These starched rollers are also peculiarly applicable in many cases of diseases of joints, where it is absolutely necessary to keep the ends of the bones at perfect rest, without the slightest motion on each other, in order to effect a cure. They answer the purpose far better than the moulded leather, and other means in common use. In Guy's Hospital they have been used in these cases, the starch being rubbed down with a solution of isinglass, instead of water. This possesses no advantage over the simple starch, and is much more sticky and uncomfortable to the patient, though superior to a nasty mess of egg and flour used in St. Bartholomew's Hospital.

The only objection, and it is a great one,

to the starch is, the length of time it requires in drying. It was lately mentioned in the wards of the hospital, that a mixture of mucilage of acacia with creta preparata, dried very much sooner, and answered all the purposes of the starch. Should trials be made with this composition, the result shall be communicated to our readers.

UNIVERSITY COLLEGE HOSPITAL.

(*Conclusion of the Case of A. P. from p.* 121.)

Sept. 23. She has been going on well since the last report, a great part of the wound has united.

28. The case has proceeded favourably, but the bowels have not been opened for the last two days, and she has refused to take purgative medicine; some erythema appeared on the right side of the face about 10, A.M.; heat of skin; thirst; pulse 100.—To have five grains of calomel and a quarter of a drop of croton oil directly. In the evening the erythema had increased, and she was ordered five grains of calomel and three of scammony. The bowels have been opened, and an offensive discharge removed. Mr. Liston ordered a twelfth of a grain of extract of belladonna every four hours. In the course of the day she had taken some tartarised antimony, which caused her to vomit, and it was, therefore, omitted.

29. The erythema has increased, and now pervades the whole of the right side of the face and upwards to the scalp; the pulse is 120, but distinct; no delirinm; perfect consciousness; thirst; heat of skin. The calomel and scammony to be repeated, and again after four hours.

Vespere. The bowels have not been moved; warm fomentations to the head; the affected parts to be sprinkled with flour; erythema extending. To have two tablespoonsful of the following mixture every two hours:—Tartarised soda and tartrate of potash, of each one ounce; tartarised antimony, one grain; camphor mixture, eight ounces. Wound not discharging.

30. The erythema and swelling have extended over the left side of the face and head; pulse 120, but distinct; bowels have been freely opened; she appears weak; she has thirst, and some heat of surface; she is very drowsy; no delirium. She is, apparently, much worse than yesterday. To take no medicine; her strength to be supported by beef tea and chicken broth.

Oct. 1. Redness and swelling of the head and face less; pulse 100; less drowsiness; bowels have been opened; wound again beginning to discharge; she appears altogether in a more favourable state than yesterday. Some œdema of the face remaining, particularly at the lower lip.

2. The swelling is rapidly subsiding; the head is free from pain. No medicine.

6. All the erythema gone. She sat up today, and appears very well.

13. Wound healed; a bandage is placed over the lower lip to lessen the œdema which is constantly present. The tongue can be protruded, and she speaks tolerably distinctly.

20. She is quite well, and there is very little disfigurement; the protrusion of the remaining portion of the jaw, on the other side, causes some inconvenience.

WEST'S DISSECTING OR SURGICAL MICROSCOPE.

A convenient little microscope has been forwarded to us by Mr. West, of Fleet-street. It seems to be well calculated for the examination of skin diseases, and will prove particularly useful to the practical botanist. The focus of the instrument is fixed, and it may be retained over any part of the body, or other object, by a very simple contrivance.

Dr. Charles J. Fox has been elected to succeed the late Dr. Thomas Davies, as physician to the Infirmary for Diseases of the Lungs.

TO CORRESPONDENTS.

P. L., Islington. The information respecting Tomlins may be forwarded, but we cannot promise to publish it. He appears to be a fit companion for the miscreants of the *Herald*.

Y. Z. The man with the rupture and well-adapted truss, would undoubtedly be in the greatest danger of the two persons.

Mercator. The College is liable to an action for detaining the diploma in question; that is, if the regular fees were paid, and if the diploma was obtained without any fraudulent representation or incorrect certificate. Probably, the whole of the circumstances have not been described by our correspondent. The question has been mislaid until this week.

We have carefully perused the documents forwarded by *Mr. Coley*, but cannot insert an *ex parte* statement of transactions, which, if faithfully represented, are most disgraceful to the parties concerned.

Mr. Gray, when he has lived longer, will, 'tis hoped, have grown wiser; a consummation which a knowledge of *the law* on the case in question, would much assist.